TECHNOLOGY QUARTERLY

AND

PROCEEDINGS OF THE SOCIETY OF ARTS.

VOL. XV. JUNE, 1902. No. 2.

PROCEEDINGS OF THE SOCIETY OF ARTS.

FORTIETH YEAR, 1901–1902.

BOSTON, January 6, 1902.

THE 560th regular meeting of the SOCIETY OF ARTS was held on this day at 8 P.M., in the Walker Building, Colonel Hewins presiding. Two hundred and fifty persons were present.

At the conclusion of the regular business, Professor George E. Hale, Director of the Yerkes Observatory, addressed the Society on "The New Star in Perseus."

The speaker referred to the discovery of the new star in Cassiopeia, by Tycho Brahe, November 11, 1572, and its influence on astronomy, likewise of the exceedingly primitive instruments available at that time for the investigation of such phenomena. Many new stars have been discovered since then, the latest being the new star in Perseus, on the evening of February 21–22, 1901. Dr. Anderson, of Edinborough, noticed on that evening in the Constellation of

Perseus a star equal in brightness to the Pole star. The brightness increased about 10,000 in three days, then began to fade away, at first gradually, afterwards fluctuating in intensity, until its final disappearance in May. Its color was white, then red. The light of the star must have been increased with great rapidity, for an examination of the photographs of this region of the heavens made on and before February 19th, at the Harvard Observatory, failed to show any trace of the star, though the plates showed stars as faint as the eleventh magnitude. A photograph of the new star with the 40″ refractor of the Yerkes Observatory was shown.

The speaker referred to the rise of spectroscopy and its bearing on the solution of astronomical problems as to the nature and condition of heavenly bodies, problems which until 1860 seemed beyond the power of investigation. The production of continuous bright and dark line spectra were explained, and slides were shown to illustrate the life-history of a star as it can be gleaned only from a study of the gradual changes in its spectrum. This method of analysis was applied to the new star during the weeks following its appearance, and the changes in physical condition which it underwent were explained by slides thrown on the screen. By the first of June the spectrum had changed to one characteristic of gaseous nebula. During these months the change in the true position of the lines in the spectrum had shown the star to be in the motion away from the earth at a small velocity determined by the Doppler principle.

On August 23d, Professor Wolf, of Heidelberg, discovered a condensation of the faint nebula near the star. No further observations were made until September 26th, when Professor Ritchey, of the Yerkes Observatory, succeeded in obtaining, after a four-hour exposure with the 20″ reflector, a photograph showing nebula surrounding the star with four principal condensations of faint nebula. Owing to cloudy weather, no further photographs were secured by him. On November 7th, Professor Perrine, of the Lick Observatory, obtained a photograph showing marked changes of position in the four pronounced condensations, the change in position corresponding to velocities of the order of the velocity of light. The entire nebulosity had expanded outward.

Some beautiful slides of nebulæ were shown, among them the great nebula of Orion, the nebula in Andromeda, and the spiral nebulæ. The latter part of the lecture was devoted to a discussion

of the various hypotheses regarding the cause of the sudden appearance of new stars.

On the motion of Professor Clifford, the thanks of the Society were extended to Professor Hale for his admirable presentation of this fascinating subject.

BOSTON, January 23, 1902.

The 561st meeting of the SOCIETY OF ARTS was held on this day, in the Walker Building, Professor Cross presiding. One hundred and sixty-five persons were present. Mr. George R. Hardy, Engineer, N. Y., N. H., & H. R. R., and Mr. Henry S. Anderson, Manager United Electric Light Company, Springfield, were elected to Associate Membership.

Mr. Calvin W. Rice, of New York City, spoke on the "Utilization of Electricity in Mines." In the early days of mining, hydraulic transmission of power was used. Later arose the pneumatic system, as a consequence of the development of the air drill and hoist, and the fact that the spent air assisted somewhat in the ventilation. Following the pneumatic system came steam, and lastly, rope driving. In all these methods the transmission is limited, not alone by distance but also on account of the scarcity of wood and coal, and at high elevations the scarcity of water. It was only natural with the development of the application of electricity to industrial work, that attention should be directed to its utilization in mining. With electric power, there is no reasonable limit to the distance of transmission, and hence it becomes possible to locate the power plant either near the water supply or else near the ore bodies.

The main progress in the use of electricity has been made during the last ten years. The first system was direct current at 250 volts. During the last five years alternating currents have been introduced for coal cutters, hoists, and pumps. The voltage has been raised from 250 to 500 volts, so as to be uniform with the street railways. Considerable discussion has arisen as to the wisdom of such high voltages, owing to the danger to life. The shafts or tunnels are very low, and the frequent explosions, together with excessive amount of water, render the insulation very poor.

One of the most valuable applications of electricity in mining is electric haulage. The engines are most substantially built to haul the required loads, and of low construction to pass through the low tunnels. Direct currents are alone used, since no successful alternating current motor for such work has yet been constructed.

The general installations in mines may be classified as follows: (1) Rope haulage and steam for other purposes; (2) electric haulage and compressed air for other purposes; (3) electric haulage and electricity for other purposes.

Many slides were shown of electric locomotives, methods of installation, and curious electrical devices. A vote of thanks was given to Mr. Rice for his interesting paper.

Boston, February 13, 1902.

The 562d regular meeting of the Society of Arts was held, in the Walker Building, on this day at 8 P.M., President Pritchett presiding. Three hundred and fifty persons were present.

The report of the previous meeting was read and approved. The following papers were presented by title: "Some Thermal Properties of Naphthas and Kerosenes," by A. H. Gill and H. R. Healy; "Apparatus for the Rapid Comparison of Voltmeters," by Professor F. A. Laws and Dr. W. D. Coolidge; "A Study of Certain Shades and Globes for Electric Lights as Used in Interior Illumination, Part 2," by William Lincoln Smith, S. B.

Professor W. H. Burr, of Columbia University, Member of the United States Commission on the Isthmian Canal, spoke on "Some Features of the Isthmian Canal Question." A *résumé* of the lecture will not be given, as the report of the Isthmian Canal Commission for 1899–1901 has been placed on the files of the Society. An interesting discussion followed.

Dr. Pritchett expressed to the speaker the gratitude of the audience for the admirable, instructive, and interesting lecture.

Boston, February 25, 1902.

The 563d regular meeting of the Society of Arts was held on this day at the usual hour, in the Walker Building, Mr. Desmond FitzGerald presiding. Three hundred persons were present.

At the conclusion of the regular business, Mr. William Barclay Parsons, Chief Engineer of the New York Subway, gave an illustrated lecture on "Engineering in China." The first part of the lecture was devoted to a consideration of the Chinese as engineers, the subject being treated under the heads of transportation, construction, and mechanics. For the inland commerce the rivers are the chief highways. Junks of the most primitive construction are used, propelled by oar, sail, and sometimes by stern paddle wheel. Although the streams are often shallow and filled with obstructions, no attempt has ever been made to render the navigation less dangerous. For coast and sea-going work, a junk of large and strong proportions is used, furnished with water-tight compartments. This bulkhead construction is known to have been used in China since the thirteenth century, and was mentioned by Mr. Parsons as one of the number of devices which are assumed to be of modern occidental origin. The land transportation into the interior is in the north by cart drawn by an ox or horse, in the south by wheelbarrow, and as a consequence there are no road systems, the highways being often almost impassable. A large part of the inland commerce is carried on the backs of coolies.

The Chinese have been acquainted, apparently, for many centuries with the principles of good engineering along static lines. This is learned from an examination of the old bridges, pagodas, and great walls. Indeed, the arch construction is of the highest order, and proves that the principles involved were known long before the time of the Romans, to whom arch construction is attributed. On dynamic lines, however, the greatest ignorance is manifested, and it will be in the development of the mines, the improvements in the methods of transportation of people and goods, and in the applications of machines of all kinds, that the foreign influence will be most felt.

The latter part of the lecture was devoted to a discussion of the plans which have been made to connect the large industrial centres of the empire. On a map it was shown that the Yangtze River, running east and west, and navigable for 1,500 miles, practically divides the Chinese empire into two halves. To connect the southern and northern halves, a concession has been granted to an American company for a railway from Canton to Hongkong, to connect with the Belgian concession between Hongkong and Peking. The English have a concession from Shanghai on the coast to Nanking, which will be

extended in time to connect with the Canton-Peking line. Rights have been granted for a line from Shanghai northward to Tientsin, and a line will ultimately be projected from Shanghai to Canton.

These proposed railway systems traverse a region rich in mineral wealth, coal, iron, petroleum, gold, silver, and other metals.

On the motion of Professor Swain, a vote of thanks was extended to Mr. Parsons.

BOSTON, March 13, 1902.

The 564th regular meeting was held on this day, in the Rogers Building, at the usual hour, Mr. Blodgett presiding. Ninety-one persons were present.

The following persons were elected to Associate Membership: C. R. Gyzander, H. L. Morse, R. R. Goodell, Ernest W. Day.

The following paper was presented by title: "A Short Method of Finding Time by Equal Altitudes of Two Stars," by George L. Hosmer.

Dr. Heinrich O. Hofman, Professor of Metallurgy at the Institute, gave a most interesting and valuable lecture on "Aluminum as a Reducing and a Heat Producing Agent." The lecture was illustrated by stereopticon and experiments. The paper will shortly appear in the TECHNOLOGY QUARTERLY.[1]

GEORGE V. WENDELL, *Secretary.*

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[1] See pages 93–104 of the current issue.

ALUMINUM AS A REDUCING AND A HEAT-PRODUCING AGENT.

By H. O. HOFMAN.

Read March 13, 1902.

Introductory.

Speaking broadly, metals can be said to occur in nature in the native state, or as sulphides (including arsenides, antimonides, etc.) or as oxides.

The metallurgical treatment of base metals in the native state is comparatively simple, as they are already in the state in which they are to be used in the arts. Taking, *e. g.*, native copper ores from Lake Superior, they are crushed and washed in order to eliminate as much as possible the valueless gangue, then the resulting copper concentrate is subjected to a process of fusion, which, at least when considered chemically, is relatively simple.

With sulphides the matter becomes more complicated. There are two ways of attacking the problem. Under the right conditions of temperature, a metal which has a stronger affinity for sulphur can be brought in contact with the sulphide, when this will be decomposed. Thus from galena (PbS), lead is extracted by means of iron, as iron has a greater affinity for sulphur than lead:

$$PbS + Fe = FeS + Pb.$$

The other method available to decompose a sulphide, is to convert it into oxide by roasting, *i. e.*, by heating with access of air. Brass-colored pyrite (FeS_2) is thus readily converted into brownish-red ferric oxide (Fe_2O_3):

$$2FeS_2 + 11O = Fe_2O_3 + 4SO_2.$$

This leads to the third form in which metals occur in nature, *i. e.*, as oxides. In order to obtain the metal from its oxide, this is usually

subjected to a reducing fusion. The common reducing agents are carbon (C) and carbon monoxide (CO). At a low temperature carbon burns to carbon dioxide (CO_2):

$$C + O_2 = CO_2,$$

at a high temperature only to carbon monoxide:

$$C + O = CO.$$

The affinity of carbon for oxygen increases with the temperature; it requires, *e. g.*, carbon heated to over 3000° C., if it is to wrest the oxygen from alumina (Al_2O_3):

$$Al_2O_3 + 3C = Al_2 + 3CO.$$

Carbon monoxide acting as a reducing agent burns to carbon dioxide:

$$CO + O = CO_2.$$

It can, therefore, do satisfactory work only at comparatively low temperatures, of 1000° C. and less.

Beside the usual reducing agents, carbon and carbon monoxide, we can use the different affinities of metals for oxygen for purposes of reduction.[1] The affinity a metal has for oxygen is measured by the amount of heat that is evolved when the metal combines with oxygen. The greater the heat evolved, the stronger will be the compound and, what is the same thing, the more difficult will it be to separate the oxygen again from the metal. As shown in the accompanying table, 1 gramme of aluminum burning to alumina evolves 7,250 calories; 1 gramme of magnesium, only 6,000 calories, and so on down the list. If we bring, *e. g.*, chromic oxide (Cr_2O_3) of a calorific value of 60 together with metallic aluminum and heat the mixture to 1010° C., at which temperature the affinity of aluminum begins to assert itself, aluminum will rob chromic oxide of its oxygen:

$$Cr_2O_3 + Al_2 = Cr_2 + Al_2O_3;$$

the temperature will rise quickly to above 3000° C., and we shall obtain fused metallic chromium and fused alumina.

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[1] Roberts-Austen. Proc. Royal Institution, 1901; Nature, 1901, 64, 360; Proc. Royal Institution, 1893-95, 14, 497; Nature, 1895, 52, 14, 39.
Keller. Jl. Am. Chem. Soc., 1894, 833.

TABLE.

HEAT EVOLVED BY BURNING ONE GRAMME OF THE FOLLOWING ELEMENTS.

Element, 1 gramme.	Product of combustion.	Calories.
Al	Al_2O_3	7,250
Mg	MgO	6,000
Ni	NiO	2,200
Mn	MnO_2	2,110
Fe	Fe_2O_3	1,790
Fe	Fe_3O_4	1,580
Fe	FeO	1,190
Co	CoO	1,090
Cu	CuO	600
Pb	PbO	240
Ba	BaO	90
Cr	Cr_2O_3	60
Ag	Ag_2O	30

ALUMINUM AS A REDUCING AGENT.

The property of aluminum to act as a reducing agent became known soon after Wöhler had isolated the metal in 1827. The earlier investigators worked more with chlorides and fluorides than with oxides. Thus Wöhler[1] in 1858 obtained crystalline compounds of chromium and aluminum; Michel[2] in 1860, compounds of aluminum with manganese, iron, nickel, tungsten, molybdenum and titanium; the brothers Tissier[3] in 1856 reduced the oxides of copper and lead; Vautin[4] in 1892 decomposed galena; Franck[5] in 1898 reduced phosphoric acid, and so on.

The first commercial application of aluminum as a reducing agent

[1] Liebig's Annalen der Chemie, 106, 118.
[2] Liebig's Annalen der Chemie, 113, 248.
[3] Comptes Rendus, 43, 1187.
[4] Journal Society Chemical Industry, 1898, 543.
[5] Stahl und Eisen, 1898, 410; Jl. Iron and Steel Inst., 1898, II, 549; Bull. Soc. Chim., Paris, 1894, 11, 439.

was made by Messrs. Green and Wahl,[1] who, between 1891 and 1893, experimented especially on the production of carbonless manganese. They manufactured considerable quantities of it in 1894–95 at the Disston Works in Tacony, near Philadelphia. A sample of manganese, presented to the Institute by Dr. W. H. Wahl, contains Mn, 96.5%; Fe, 3.0%; Si, 0.5%. The metal is dense and homogeneous; it has a steel-gray color, with a pinkish tint; it is hard and brittle; becomes fissured when cast in iron moulds, but does not oxidize or disintegrate in moist air, as does all manganese that has been reduced by carbon. Such a manganese retains considerable amounts of carbon on account of the strong affinity of manganese for carbon. Work at Tacony was stopped on account of a terrific explosion, which destroyed the plant. The then high price of aluminum made the cost of manganese too high, so the works were not rebuilt. Messrs. Green and Wahl worked along the lines of the earlier investigators, who carried on their experiments in crucibles heated in furnaces. The reaction, when once started, was often so violent as to blow the whole charge out of the crucible. The explosion at the Tacony Works was due, probably, to some violent reaction taking place in the reduction process.

By modifying the mode of operating, Dr. Hans Goldschmidt, of Essen, Germany, has taught us how to control the violence of the reaction. His important process will be discussed later on, and we will confine our attention for the present to the action of aluminum as a reducing agent pure and simple.

Taking, *e.g.*, *nickel*,— its melting-point lies at 1450° C.[2] In melting it down in a crucible, a superficial oxidation can hardly be prevented, and some of the nickelous oxide formed is taken up by the nickel and makes it brittle. Therefore, until about twenty-five years ago, most of the nickel in the market was in the form of small welded cubes. Nickelous oxide was mixed with sugar, starch, or a similar reducing agent, formed into cubes, dried, and brought in a closed vessel to a white heat, when the reduced metallic nickel particles readily welded together. Broken nickel is prepared in a similar manner. In 1876 Mr. Jos. Wharton, of Camden, New Jersey, exhibited at the Centennial Exposition of Philadelphia, nickel in the form of cast ingots, slabs, etc. He had added the necessary small amounts of magnesium

[1] Garrison. Transactions American Institute of Mining Engineers, 21, 887.
[2] Styffe. Oesterreichische Zeitschrift für Berg- und Hüttenwesen, 1894, 340.

to the crucible-charge, which reduced the nickelous oxide, the magnesium being converted into magnesia. In the absence of nickelous oxide, it was an easy matter to melt and cast nickel. Today nickel anodes in the form of slabs are a regular article of commerce. Magnesium has to some extent been replaced by aluminum, which, according to the table, is more powerful.

In making *brass* in a crucible, the copper is first charged, covered with charcoal and melted down; then zinc, previously warmed, is added in small amounts at a time in order to prevent cooling. When the necessary quantity has been charged, the alloy is heated to the pouring temperature, which lies near the ignition point of zinc, and is thoroughly stirred. The charcoal cover prevents most of the oxidation, but not all of it. An addition of 0.05—0.10% aluminum counteracts all oxidation completely; the brass will flow more freely, and will be stronger and tougher, even if no aluminum whatever remains in the alloy.

The Roessler-Edelmann Process. A very interesting application of the reducing effect of aluminum has been made by Messrs. Roessler and Edelmann, of Frankfort s. M., Germany, in desilverizing argentiferous lead by means of zinc (Parkes Process). Here from 1 to 2% zinc is stirred into lead, melted down in a kettle having a capacity of thirty to sixty tons. A zinc-silver-lead alloy forms, which, having a higher melting-point and a lower specific gravity than the lead, solidifies before the lead and rises to the surface, when it can be skimmed off from surface, as is cream from milk, and worked up separately. In heating the lead (containing say 1% silver) to well above the melting-point of zinc (412° C.) and stirring in the zinc, a superficial oxidation of both lead and zinc cannot be prevented. The result is that the zinc-silver-lead alloy contains not more than 5% silver, because a large amount of unalloyed and partly oxidized lead is carried off with the skimming. By adding 0.5% aluminum to the zinc, Roessler-Edelmann obtained a zinc-silver-lead alloy with as much as 20% silver. This modification of the Parkes Process was in operation for several years at Lautenthal, Harz Mountains, Prussia, and at Hoboken, near Antwerp, Belgium.

Galvanizing.[1] A similar favorable effect of aluminum is seen in its use in galvanizing, *i. e.*, coating iron or steel sheets with zinc. Suitably prepared iron sheets are drawn through a bath of melted

[1] Richards. Aluminum-zinc. Jl. Franklin Instit., 1901, 151, 445.

zinc which slowly becomes oxidized, forms drosses, and loses its power to coat iron. By adding one pound of an alloy of zinc with 2% of aluminum to the zinc bath, this becomes more fluid; a given weight of zinc, thus charged, coats 20% more surface, the sheets are brighter, and keep their color better. The amount of aluminum added is only 0.001%, but it has this remarkable effect.

Aluminum and Iron. The effects that small amounts of aluminum have on the different forms of iron are of great importance. It was discovered by Wittenstrom in the 80's that, if 0.05 to 0.1% of aluminum was added to a charge of wrought iron heated to pastiness, the iron liquefied so that it could be poured. The castings, so-called Mitis castings, have similar properties to wrought iron, excepting that they lack the fibrous structure.

In making steel castings, an addition of 0.03 to 0.05% of aluminum has a very beneficial effect in reducing the small amounts of iron oxide that are formed, especially in pouring. The elimination of oxides makes the steel more tough, and by preventing the oxides from oxidizing some of the carbon of the steel, it hinders the evolution of carbon monoxide, which, stirring up the steel, causes the expulsion of occluded gases; it thus diminishes, if it does not prevent, the formation of blow-holes.

The effect of 0.01 to 0.05% of aluminum upon cast iron is to convert some of the combined carbon into graphitic carbon. It is mentioned here in connection with wrought iron and steel as the third form of industrial iron. A satisfactory explanation has so far not been offered.

In the examples given, the amounts of aluminum used are very small in comparison with the quantities of base metals that are to be cast; the reason being, of course, that only small percentages of oxides have to be reduced. When the oxides increase in amount, the aluminum required for reduction grows correspondingly. This leads to the use of aluminum as a reducing agent for refractory oxides.

Reduction of Refractory Oxides. Moissan,[1] in 1894, was the first to use an aluminum bath in an experimental way. He melted aluminum down in a crucible, added slowly the oxide to be reduced, and then formed an alloy of the reduced metal with aluminum. When cold, he separated the alloy from the slag, then melted it down again,

[1] Goldschmidt. Zeitschrift für Electrochemie, 4, 494.

and, working backward, added sufficient oxide to the melted alloy to oxidize the aluminum, and thus obtained the pure metal.

The Rossi Process. Mr. A. J. Rossi,[1] of New York City, who for a number of years has been working upon the industrial reduction of titaniferous iron ores, was the first to use an aluminum bath as a reducing agent for this class of ores in a commercial way. In his plant at Buffalo, New York, he uses an electrical furnace of the well-known Siemens type. It consists of a large block of agglomerated graphite with a central cavity to form the crucible; the block is encased in a wrought-iron shell, forming the cathode; the anode is a carbon block that can be raised and lowered. The crucible is charged with aluminum in waffles, or in the form of ingot or scrap, some scrap iron is added, the anode is lowered to make contact with the metal, the current is turned on, and the charge (titaniferous iron ore) shoveled in. The iron and aluminum soon become liquid, the aluminum reacts upon the titanium ore, fumes of alumina are given off, the temperature rises to such a degree that it becomes necessary to moderate the current; later, however, it has again to be increased to keep the slag and alloy molten. The contents of the furnace are now tapped and the furnace is recharged. In from one and one half to two hours a cake of alloy of 250 pounds is thus produced. The slag assays 80 — 85% Al_2O_3 and contains 90% of the aluminum charged. The alloys made show a range of Ti, 10 — 75%; C, 0.1 — 0.8%; S, — 0.015%. The slag forms an excellent abrasive material. The alloy is hard, scratches glass, and is silver white; when low in titanium, it has large, brilliant crystalline facets; when high in titanium, it is fine-grained and less lustrous; the higher the percentage of titanium the more brittle is the alloy.

An addition of 4 to 5% of ten to twelve per cent. alloy to cast iron gives this an increase of transverse strength of about 35%, and of tensile strength of 30 to 60%.

With steel, experiments have so far been confined to crucible steel with C, 0.4%; an addition of even small amounts decidedly increases the ductility.

There is no reason why this method of reduction by means of aluminum should not be extended to other ferro-metals, such as ferro-molybdenum, ferro-chrome, ferro-chrome-molybdenum. Mr. Rossi

[1] The Mineral Industry, ix, 715.

has produced copper-titanium alloys with 5, 8, 10, and 15% Ti; the last one scratches glass easily.

The method of reducing refractory oxides by means of aluminum in an electrically heated furnace thus appears to have a good future before it, as long, of course, as the cost of aluminum, thirty-two cents per pound, is not too high, and the current sufficiently cheap.

The Hall Alumina Process.[1] Another interesting process in which aluminum figures as a reducing agent in an electrically heated furnace is that by Mr. C. M. Hall, of Niagara Falls, New York, the inventor of the Hall Aluminum Process, by means of which all aluminum of this country is produced. The raw material which furnishes the pure alumina for the electrolytic reduction of aluminum is bauxite with, say, Al_2O_3, 60%; Fe_2O_3, 18%; SiO_2, 2 to 3%; TiO_2, 3 to 4%; H_2O, 17%. This ore is usually mixed with sodium carbonate, heated to fritting in a reverberatory furnace, when sodium aluminate soluble in water is formed, while the other compounds remain insoluble. The fritted mass is leached with water; from the sodium aluminate solution, aluminum hydroxide is precipitated with carbon dioxide, and the original sodium carbonate regenerated. The aluminum hydroxide is filtered, washed, and calcined.

Mr. Hall now purifies the bauxite by a cheaper method (the Hall Alumina Process). The mineral is mixed with 8 to 10% of fifty-per-cent. ferro-aluminum, and the mixture then charged into an electrical furnace, similar in principle to the Siemens furnace, and held in fusion for about one and one half hours. The impurities, silica, ferric oxide, and titanic oxide, are reduced more or less completely to the native state, and, combining with the iron of the ferro-aluminum, sink to the bottom of the reduction vat. Silica and ferric oxide can be satisfactorily eliminated; less so, titanic oxide. The purified alumina differs, of course, from that obtained by the combined dry-and-wet method. It is grayish white, crystalline, has a specific gravity of 3.73 to 3.93; it resembles in structure a coarse-grained sandstone that is easily crushed, and often even friable between the fingers. It is partly soluble in hydrochloric acid and caustic alkalies, with liberation of methane and hydrogen; the former is probably due to an aluminum carbide, the latter to a partial reduction of Al_2O_3 to Al_3O_4, or Al_2O. The alumina furnished by this process is suited for making

[1] Townsend. Electric. World and Eng., July 6, 1901, p. 15.

a high grade of aluminum, and the process is, of course, a great deal cheaper than the usual combined dry-and-wet method. The alloy of iron, silicon, and titanium is, for the present at least, a waste product, unless researches similar to those of Mr. Rossi should procure a market for it.

The Goldschmidt Process.[1] The latest process is that of Dr. Hans Goldschmidt, of Essen, Germany. He uses aluminum as a reducing and a heating agent, and in many cases can manufacture from the resulting alumina a satisfactory abrasive material. This process has created quite a stir in the technical world, and has already an extensive literature. Even a new word, "aluminothermie" (aluminothermics), has been coined for it, in that it forms a new departure in reducing operations.

In this process, just as in the early experiments, aluminum in a more or less finely divided state is mixed with a metallic oxide and then charged into a crucible, which here is lined with magnesia. While the early experimenters heated the crucible in a furnace until the whole charge had been brought to the ignition point of aluminum, Goldschmidt found that it was only necessary to start the reaction at one point of the charge. The reason for this is that the heat liberated is sufficient for the reaction to go on without any external heating at all, when it has once been started. From the starting point, the heat works outward, and in a few minutes it has passed through the entire mass and causes the whole charge to react. In order to start the reaction, a fuse of very inflammable material is inserted into the

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[1] Goldschmidt. Ann. d. chemie, 1898, **301**, 19.
—— Verh. d. Vereins z. Bef. des Gewerbefleisses, 1899, 57; Jl. Iron and Steel Inst., 1899, II, 475.
—— Z. Verein Deutsch. Ing., **42**, 1019—1022; Jl. Iron and Steel Inst., 1898, II, 548.
—— Stahl und Eisen, 1898, 468; Jl. Iron and Steel Inst., 1898; II, 387; 1900, 567, 612; 1901, 23, 546, 560.
Lange. Jl. Iron and Steel Inst., 1900, II, 191.
Goldschmidt. Z. Electro-chemie, **7**, 935; Stahl und Eisen, 1901, 1155.
—— Z. Electro-chemie, **4**, 494 (Iron Age, 1898, October 6, p. 2).
—— Trans. Inst. Mining Eng., 1900, **19**, 411; Z. Angew. Chemie, 1900, No. 37, 919.
—— Z. Electro-chemie, 1899, **6**, 53.
—— Glück Auf, 1900, **36**, 466.
Archdeacon. Eng. Min. Jl., **70**, 219.
Duboin & Gauthier. Compt. Rend., **129**, 217: Jl. Iron and Steel Inst., 1899, II, 476.
Clerc. Bulletin de l'Industrie Minérale, 1901, XV³, 573.
Vanderheym. Bull de l'Ind. Min., 1901, XV, 627.

mixture and ignited. The fuse consists of a mixture of finely divided aluminum and barium peroxide formed into a paste; it is supplied with a magnesium ribbon, serving as a squib. The highly inflammable material, when ignited, produces locally the intense heat necessary to start the reaction.

The reduced metal will be free from aluminum so long as the crucible charge contains a slight excess of oxide over that which can be reduced by the aluminum added.[1]

Professor Kupelwieser,[2] of Leoben, Austria, has calculated the following heat-balance in reducing ferric oxide by means of aluminum and carbon:

TABLE.

THE REDUCTION OF Fe_2O_3 TO Fe BY Al AND BY C.

	Al.	C.
Compound produced	Al_2O_3	CO
Amount of reducing agent required to produce 1 kgr. of iron	0.484 kil.	0.321 kil.
Amount of heat produced by oxidation of the reducing agent	3,456 cal.	770 cal.
Heat required to reduce Fe_2O_3	1,796 cal.	1,796 cal.
Heat required for fusion of slag	548 cal.	....
Heat required for fusion of iron	362 cal.	....
Total heat required	2,706 cal.	1,796 cal.
Residual heat available	+ 750 cal.	— 1,026 cal.

In the aluminum-reduction there is available 750 calories; in the carbon-reduction there is a deficit of 1,026 calories.

Carbonless chromium and manganese produced by the Goldschmidt Process are of importance.[3] Pure chromium is almost incorrodible and has valuable electrical properties. Its main interest to the metallurgist lies in its use for making chrome steels, which so far contain

[1] The lecturer here carried out the experiment with ferric oxide and aluminum.

[2] Oesterreichische Zeitschrift für Berg- und Hüttenwesen, 1899, 145; Jl. Iron and Steel Inst., 1899, II, 475.

[3] The lecturer presented specimens.

only 0.5 to 1% Cr. The ferrochromes (with 40 to 60% Cr) ordinarily used contain ten to twelve parts carbon to one part chromium, the chromium being present as a carbide. The addition of ferrochrome, therefore, incidentally raises the carbon content of the steel and makes it impossible to obtain chrome steels high in chrome and low in carbon. While the addition of the pure chrome to open-hearth or crucible steel is advocated, it seems probable that, instead of using expensive pure chromic oxide (Cr_2O_3), it will be found more convenient to use the cheaper mineral, pure chromite ($FeO.Cr_2O_3$) with 68% Cr_2O_3, which will be reduced by aluminum, and the carbonless ferrochrome then added to the charge.

It is claimed by Goldschmidt that in adding pure chromium to the steel-charge there is a smaller loss in chromium by slagging than when ferrochrome is used,[1] but this is contradicted by Blackwell.[2]

Manganese is of less interest to the steel smelter than to the non-ferrous metallurgist, in that the ferro-manganese (80% Mn) commonly used serves its purpose and is very much cheaper than the pure manganese of the Goldschmidt process. The costs are about as 1 : 10.

In making copper-manganese alloys the use of pure manganese is important, because in using ferro-manganese (80% Mn; 8 to 9% Fe) some iron always combines with the copper. Thus a cupro-manganese, with 25 to 30% manganese, retains 2.5 to 4% iron, which has a harmful effect.

In melting down nickel, manganese can take the place of magnesium or aluminum; in the manufacture of brasses and bronzes additions of $\frac{3}{4}$ to 3% of manganese show favorable effects. Manganese further is readily alloyed with zinc and tin; thus the alloys, 20% Mn and 80% Zn, and 50% Mn and 50% Sn, are used in the arts.

An alloy, 84% Cu, 12% Mn, 4% Ni (manganin), is used for electrical resistance coils; again, an alloy of 70% Mn and 30% Cr is alloyed with copper to form chrome-manganin, and so on.

Goldschmidt has produced with his process valuable alloys of iron and titanium with 20 to 25% Ti; of manganese and titanium with 30 to 35% Ti; of lead and barium with 30% Ba, showing thus also the reduction of some alkaline earth metals.

The slag produced in the process, resembling the purified alumina

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[1] Stahl und Eisen. 1901, 547.

[2] Electro-chemist and Metallurgist, I, 196.

of Hall, is a valuable substance, in that it is an artificial corundum, which next to diamond is the hardest mineral. The slag is used as an abrasive, as a crucible-lining, and may serve as raw material for producing aluminum by electrolysis.

In the Goldschmidt process, so far, the main object has been the reduction of a refractory metallic oxide, the heat generated serving to liquefy the reduced metal and the resulting slag. The process is being applied, also, simply as a heat producer, the heat being utilized for welding, soldering, annealing, etc., where it is not convenient to use other methods of heating. The aluminum in this case need not be pure; the cheapest source of oxygen will be ferric oxide. In order to retard the reaction and thus reduce the temperature, it is necessary to add a diluent to the charge, *e.g.*, silica, lime, or magnesia. This acid or basic flux combining with the Al_2O_3 will form a slag that surrounds the part to be heated. It is removed by tapping with a hammer when cold. The amount of reagent to be used and the degree of dilution desired will depend upon the melting-point, the size and conductivity of the metal or alloy to be heated. An excess of charge would melt the metal or alloy; an insufficient amount would not bring it up to the required temperature.

This method of heating has found very wide application, and may prove even more important than the reduction of refractory metallic oxides.[1]

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[1] Applications of the process made in welding pieces of wrought iron and steel, and in patching castings, were shown by means of illustrations thrown upon the screen.

A STUDY OF SELF-PURIFICATION IN THE SUDBURY RIVER.

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Introductory.

Few sanitary problems have been debated with less conclusive results than those relating to the extent of the self-purification which occurs in streams, and to the agencies involved. Twenty-five years ago almost implicit faith was placed in the efficiency of this process to render sewage-polluted water potable even in a comparatively short distance. For example, Chapter 80, Section 96, of the Public Statutes of Massachusetts, enacted in 1878, prohibited the discharge of polluting material into a stream or pond used as a source of water supply, only "within twenty miles above the point where such supply is taken." The changed opinion of the present day was expressed by Professor W. T. Sedgwick, in 1899, when he said of the Allegheny River at Pittsburgh: "Such is the peculiar character of the river and such the character of the watershed, that infection introduced at much more remote points than Oil City (113 miles above), may, in my opinion, be actively dangerous to the health of Pittsburgh."[1]

There is little question nowadays that self-purification in rivers is practically never so complete as to make such bodies of water safe sources of water supply without preliminary treatment. At the same time it is obvious that a very considerable, if only partial, purification does occur, both as regards the removal of organic material and the destruction of bacteria. The work of Professor E. O. Jordan on the bacterial purification of the sewage of Chicago in the great

[1] Report of the Filtration Commission of the city of Pittsburgh, Pennsylvania. 1899. P. 19.

drainage canal and the Illinois River, has drawn particular attention to this subject of late.[1, 2] In Jordan's earlier paper, as well as in one published four years ago by Hammerl,[3] are elaborate reviews of earlier work on the results of "self-purification" and its contributory causes. References to this literature will be found in the bibliographies contained in the papers referred to; the general results of the observations recorded showing that the problem is a complex one, and that each case must be considered *sui generis*. Schlatter, for example, found a complete self-purification in the Limmat, only 6 miles below Zurich; Frank could not detect the sewage bacteria of Berlin after an 8-mile passage through the Havelsee. Prausnitz recorded a removal of 50 per cent. of the germs contributed to the Isar by the city of Munich in a flow of 12 to 16 miles, occupying about 8 hours. On the other hand, Kruse and Lossen could not find evidence of any marked improvement in the Rhine, 16 miles, or about 5 hours' flow, below Cologne. Hammerl, using the presence of the colon bacillus, only, as an indicator, found that the sewage of the towns along the Mur disappeared at various distances, ranging from 2 miles up to 20. Hulwa recorded the complete chemical purification of the Oder by a flow of 20 miles below Breslau. Heider could detect the sewage of Vienna in the Danube at a point 24 miles below, by bacterial, but not by chemical, methods. In Jordan's work it appeared that while only one-half the bacteria introduced in the Chicago sewage was removed by a 29-mile flow through the drainage canal from Bridgeport to Lockport, at a rate of from $\frac{1}{2}$ to $\frac{9}{10}$ mile per hour, a flow of 27 miles between Lockport and Morris reduced the bacteria by 90 per cent., and a flow of 24 miles below Morris, at a rate of $\frac{1}{2}$ mile per hour, caused the complete disappearance of the excess of bacteria introduced at that place. Finally, at the opposite extreme to the favorable results of Schlatter and Frank, stand the observations of Durand-Claye, who found that a 60-mile flow of the Seine was required to remove all chemical evidence of the presence of the sewage of Paris. Altogether it is

[1] Jordan, E. O. Some Observations upon the Bacterial Self-Purification of Streams. Journal of Experimental Medicine, V, 1900, 271.

[2] Jordan, E. O. The Relative Abundance of Bacillus coli communis in River Water as an Index of the Self-Purification of Streams. Journal of Hygiene, I, 295.

[3] Hammerl, H. Ueber das Vorkommen des Bacterium coli im Flusswasser. Hygienische Rundschau, VII, 1897, 529.

clear that the rapidity and completeness of the purifying process varies very widely with local conditions.

The Sudbury River.

The Sudbury River, in the eastern part of Massachusetts, furnishes, in some respects, a particularly favorable opportunity for the study of self-purification. Of its total watershed of 164 square miles, the upper portion, including 94 square miles of steep and hilly country, is used as the source of part of the water supply for the Metropolitan District of Boston. The Legislative Act under which this water privilege was granted provided that 1,500,000 gallons of water per day should be allowed to flow past the lowest point at which water was taken by the city; and in dry weather the flow of the river immediately below scarcely exceeds this amount. From the reservoir of the Metropolitan Waterworks to the village of Saxonville, the stream is practically unpolluted, except at times of very heavy rain. At Saxonville it is dammed to furnish power for the Saxonville Mills, and from the mill grounds it flows off, heavily charged with manufacturing wastes. Measurements of these waste liquors, made by the Massachusetts State Board of Health in 1900 and 1901, showed their amount to be about 30,000 gallons per day, and this may now have increased somewhat on account of enlargements in the mills. The wastes, when studied by the Board, were made up of about 21,000 gallons of wool-scouring liquor, 7,000 gallons of dye liquor, and 2,000 gallons of yarn-washing liquor. The analyses made by the Board of Health[1] showed their composition to be as follows:

[1] Report of the State Board of Health (of Mass.) upon the Sanitary Condition of the Sudbury and Concord Rivers and the meadows adjacent thereto. May, 1901.

AVERAGES OF ANALYSES OF WASTE LIQUORS FROM WOOL SCOURING, CLOTH WASHING, AND DYEING, AT SAXONVILLE.

[Parts per 1,000,000.]

	Residue on Evaporation.			Ammonia.				Chlorine.	Oxygen consumed.
					Albuminoid.				
	Total.	Loss on ignition.	Fixed.	Free.	Total.	In solution.	In suspension.		
No. 1 . .	14,880	7,180	7,700	102.7	67.6	—	—	—	1,200
No. 2 . .	16,960	11,030	5,930	61.0	120.7	58.8	61.9	255.6	1,362
No. 3 . .	20,830	17,550	3,280	3.0	154.8	87.0	67.8	279.0	2,810
No. 4 . .	3,410	2,580	830	0.5	13.0	—	—	—	498
No. 5 . .	26,220	12,310	13,910	5.6	101.0	12.0	89.0	350.0	1,780
No. 6 . .	3,370	810	2,560	9.6	23.6	22.6	1.0	204.0	102

NOTE: No. 1 is an average of samples collected November 16, 1900, of wool-scouring liquors. No. 2, average of all samples of wool-scouring liquors. No. 3, average from first bowl of yarn-washing liquor. No. 4, average from second bowl of yarn-washing liquor. No. 5, average of cloth-washing liquor. No. 6, sample of waste liquor from dyeing.

Besides the manufactural wastes from the mills, a certain small amount of domestic sewage probably enters the river from the village of Saxonville. Below the village, however, beyond the station marked 10 on the accompanying plan (Figure 1), the stream flows for 16 miles to the town of Concord without any appreciable additional pollution. For the first 3 miles below Saxonville it runs rather rapidly, in places perhaps at a rate above 2 miles per hour. Beyond Station 9 on the plan, the river enters the Wayland and Sudbury meadows, and for the rest of its course winds sluggishly along through a weedy channel with a speed of not over $\frac{1}{4}$ mile per hour. There are no dwellings, with the exception of half a dozen summer camps in the proximity of the river, in all this distance; and it is entered by only a few small tributary brooks.

We have, then, in the Sudbury River, during the dry season, a small stream polluted at a single point to an extent of about 2 per cent. of its total volume by a liquid three to five times as strong as ordinary city sewage. For 16 miles below this point no additional pollution enters, and (at times of low water) comparatively little dilution occurs. For most of this distance the flow is very sluggish, and

the conditions for self-purification decidedly favorable. The report of the Massachusetts State Board of Health, already quoted, contains analyses of samples taken from seven points between Saxonville and Concord on August 15, 1900, from eight points on August 29, 1900, and from five points on February 27, 1901. In general, these analyses showed a marked purification; and in commenting upon them the engineer of the Board noted that near the outlet of Hurd's Pond, 4 miles below the Saxonville dam, half the pollution as measured by free and albuminoid ammonia disappeared, and that at Canal Bridge, $7\frac{1}{2}$ miles below the dam, practically all evidence of pollution was removed. No bacteriological analyses, and no determinations of dissolved oxygen, were made upon these samples. The object of the present investigation was to extend the study of the State Board of Health by making complete chemical and bacteriological analyses of a more extended series of samples taken at the time of minimum flow of the stream, when the conditions for the study of self-purification are most favorable.

All field-work and collection of samples has been done by Messrs. Winslow and Hansen: all chemical work by Mr. Woodman; and all bacteriological work by Mr. Winslow. The figures have been drawn by Mr. Hansen. For the conclusions the authors are alike responsible.

Collection of Samples.

The first set of samples, ten in number, were collected October 25, 1901, the first sample, at Concord, being taken at 11 o'clock A.M., and the last, above the Saxonville dam, about 8 P.M. The position of the stations is shown on the plan (Figure 1). Station 1 was about 16 miles below the dam and just above the first houses of the town of Concord; Station 2 was at the outlet, and Station 3 at the inlet, of Fairhaven Bay, the only considerable pond on this part of the stream; Stations 4 to 7 were at points 2 to 3 miles apart in the course of the stream through the meadows; no sample was taken at Station 8 on this first day; Stations 9 and 10 were at bridges, respectively 2.7 miles and 3,000 feet below the Saxonville dam; no sample was taken on this day at Station 11, 150 feet below the entrance of the raceway from the mill; Station 12 was in the mill-pond, just above the dam. One week later, November 1, a second set of samples was taken, covering all the twelve stations indicated on the chart, but

in the inverse order, beginning at Saxonville at 9 A.M. and ending at Concord at 5 P.M. The weather on both days was clear, except for a shower in the afternoon of November 1, beginning about 3 P.M.; and for several weeks previously there had been very little rain. The temperature of the water was taken on the second day and varied only between 11° and 12° C., except at Station 11, just below the mill, where it was 18°. It was not possible to make measurements either of the amount of water flowing in the river or of the velocity. As regards the latter point, however, it is certain that $\frac{1}{4}$ mile per hour would be an outside estimate of the speed below Station 8. At each station three samples were taken, one for bacteriological analysis, one for chemical analysis, and one for the determination of the dissolved oxygen; and each sample was collected in three portions, from each side and from the centre of the stream. Inspection at the time of taking the samples showed the existence of pollution evident to the eye above Station 6, but no farther down the river. At Station 11 the stream, which had almost all passed through the mill, was highly discolored with dye liquor and very offensive. At Station 10 the milky look had somewhat disappeared, but the water was obviously foul; and between this point and Station 9 the stream flowed swiftly over a bed of rocks largely overgrown with Leptomitus, or some other sewage fungus. Between Stations 9 and 7 the speed of the river slackened, and an opportunity was furnished for sedimentation. At Station 6 the last ocular evidence of contamination was noticed, consisting of a considerable amount of flocculent suspended material in the water.

Bacterial Analyses.

The analysis of the bacterial samples was begun on the evening of collection Gelatin plates were made in duplicate and kept at room temperature, and agar plates were made and incubated at 37° to give some idea of the proportion of thermophiles present. The agar plates were counted after 18 hours, the gelatin plates after 60 hours. Four or five portions of each sample were incubated in dextrose broth to determine the presence of *B. coli*. In the first set of samples portions of 1 c.c., $\frac{1}{10}$, $\frac{1}{100}$, and $\frac{1}{1000}$, and in the polluted portions of the river, of $\frac{1}{10000}$ c.c., were used. In the second set of samples, a definite portion was selected for each station, $\frac{1}{100}$ c.c. for Stations 10 and 11, $\frac{1}{10}$ for Stations 8 and 9, and 1 c.c. for the

other points, and five duplicate tubes were inoculated. From the tubes showing fermentation of dextrose, litmus-lactose-agar plates were made; and if red colonies appeared, three were fished from each plate and sub-cultures made. Only those organisms giving typical reactions in dextrose broth, milk, nitrate solution and peptone solution, in the gelatin stab and on the agar streak, were considered to be colon bacilli.

Chemical Analyses.

The methods of chemical analysis followed were in the main similar to those employed in the systematic examination of water supplies by the Massachusetts State Board of Health. They are quite fully described in published reports of the Board, and it will be necessary to mention specifically only a few minor points.

The chemical analysis was begun on the morning following the collection of the samples. A portion of the sample was filtered through washed Swedish filter paper, and the filtered water was used for the determination of residue on evaporation, oxygen consumed, and color. The albuminoid ammonia was determined on both the filtered and unfiltered water, thus giving the amount in solution and suspension.

Color was determined by comparison with natural water standards, and is expressed in terms of the standard ammonia solution. Nitrites were estimated by Ilosvay's modification of the Griess method after decolorization with milk of alumina. Oxygen consumed was determined by Kubel's hot acid method, adding the reagents cold and boiling for five minutes. The figures given for the chlorine represent the average of closely agreeing duplicate determinations. Oxygen dissolved was determined by Winkler's method on special samples collected at the same time as the samples for the main analysis.

TABULATION OF RESULTS.—TABLE I.

CHEMICAL ANALYSES.—SERIES I, OCTOBER 25. (Figures 2 to 6.)

(Parts in 1,000,000.)

Station.	Date of		Physical.					Residue on Evaporation.			Nitrogen as						Oxygen consumed.	Hardness.	Chlorine.	Oxygen. Per cent. saturation.
	Collection.	Examination.	Color.	Turbidity.	Sediment.	Odor.		Total.	Loss on ignition.	Fixed.	Alb. ammonia.			Free ammonia.	Nitrites.	Nitrates.				
						Cold.	Hot.				Total.	Sol'n.	Susp.							
	1901.	1901.																		
1	Oct. 25	Oct. 26	.95	V. sl.	V. sl.	Dist. veg.	Dist. veg.	63.5	22.5	41.0	.240	.214	.026	.020	.003	.070	9.24	18.2	5.55	68.34
2	"	"	.93	"	"	Dist. veg. and f. musty	" "	64.0	24.0	40.0	.262	.218	.044	.032	.002	.080	9.41	16.9	5.35	65.40
3	"	"	.93	"	"	" "	" "	62.0	21.0	41.0	.224	.176	.048	.044	.002	.090	9.13	18.2	5.24	61.91
4	"	"	.70	"	"	" "	" "	66 5	22.5	44.0	.230	.194	.036	.034	.004	.090	8.23	18.2	5.60	61.24
5	"	"	.75	"	"	Dist. veg. and musty	" "	61.0	20.0	41.0	.232	.198	.034	.034	.003	.050	8.15	18.2	5.30	60.53
6	"	"	.70	"	Sl.	" "	" "	86.0	24.5	61.5	.224	.186	.038	.018	.002	.070	7.78	22.1	5.37	64.21
7	"	"	.95	Sl.	"	Veg. and unpleas.	Veg. and unpleas.	65.0	24.0	41.0	.258	.228	.030	.034	.004	.100	8.83	15.6	4.60	54.64
8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
9	"	"	.95 turbid	Dist. and woolly threads	Sl. to consid. brown	"	Dist. veg.	63.5	23.5	40.0	.292	.214	.078	.028	.003	.110	9.51	15.6	5.08	56.40
10	"	"	.97 turbid	"	Cons. floc. and woolly	"	"	65.0	28.5	36.5	.338	.226	.112	.032	.005	.120	10.19	14.3	4.70	60.74
11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
12	"	"	.93	V. sl.	V. sl.	Dist. veg. and f. musty	"	51.5	22.0	29.5	.236	.184	.052	.014	.002	.100	8.76	12.7	3.70	64.49

TABLE II. — CHEMICAL ANALYSES. — SERIES II, NOVEMBER 1. (Figures 2 to 6.)

(Parts in 1,000,000.)

Station.	Date of		Physical.					Residue on Evaporation.			Nitrogen as						Oxygen consumed.	Hardness.	Chlorine.	Oxygen. Per cent. saturation.
	Collection.	Examination.	Color.	Turbidity.	Sediment.	Odor.		Total.	Loss on ignition.	Fixed.	Alb. ammonia.			Free ammonia.	Nitrites.	Nitrates.				
						Cold.	Hot.				Total.	Sol'n.	Susp.							
	1901.	1901.																		
1	Nov. 1	Nov. 2	.60	None	V. sl.	F. veg.	Dist. veg.	63.0	23.5	39.5	.186	.160	.026	.030	.002	.140	7.06	18.2	4.65	71.95
2	"	"	.55	"	"	"	" "	62.5	23.0	39.5	.176	.160	.016	.044	.002	.120	7.14	16.9	4.65	67.02
3	"	"	.45	V. sl.	Sl.	Dist. veg.	" "	61.5	21.0	40 5	.182	.156	.026	.054	.004	.180	6.16	18.2	5.19	65.80
4	"	"	.50	"	V. sl.	" "	" " and f. grassy	65 0	22.0	43.0	.198	.186	.012	.036	.003	.180	6.90	18.2	5.19	62.30
5	"	"	.50	"	Sl.	" "	Dist. veg.	65.0	21.5	43.5	.200	.164	.036	.018	.003	.130	6.75	18.2	5.11	59.55
6	"	"	.48	Sl.	Sl. to cons.	" "	" "	65.0	20.5	44.5	.192	.146	.046	.026	.004	.180	6.61	19.5	5.44	63.70
7	"	"	.48	V. sl.	Sl.	" "	" "	56.0	18.5	37.5	.208	.180	.028	.032	.004	.120	7.50	18.2	4.65	56.58
8	"	"	.65	Sl.	"	" " and f. musty	" "	65.0	23.5	41.5	.204	.180	.024	.022	.004	.170	7.53	19.5	4.95	49.63
9	"	"	.70 turbid	Dec. floc.	Sl. to cons.	Dist. veg. and musty	" " and f. unpleas.	87.0	30.0	57.0	.344	.290	.054	.004	.011	.150	8.70	23.4	7.43	26.02
10	"	"	.55	Dist. floc.	Sl.	Dist. veg. and unpleas.	Dist. veg.	76.0	25.5	50.5	.304	.194	.110	.002	.010	.270	7.38	24.7	7.08	48.39
11	"	"	.95-1.00	Dist. to Dec.	Sl. to cons.	" "	" "	92.0	34.0	58.0	.572	.358	.214	.012	.002	.070	11.63	14.3	4.85	33.73
12	"	"	.93	V. sl.	Sl.	Dist. veg. and f. musty	" "	49.5	19.5	30.0	.164	.152	.012	.022	.001	.120	8.25	13.5	3.20	74.18

TABLE III. (Figures 6 and 7.)
BACTERIA PER CUBIC CENTIMETER.

STATION.	SERIES I, OCT. 25.		SERIES II, Nov. 1.	
	Gelatin, 20°.	Agar, 37°.	Gelatin, 20°.	Agar, 37°.
1	180	46	170	18
2	300	33	390	29
3	770	36	720	29
4	4,230	27	1,320	37
5	6,400	44	1,390	39
6	8,100	70	1.900	72
7	7,100	130	2,900	71
8	—	—	4,600	43
9	17,000	610	73,000	870
10	6,000	1,400	33.000	4,300
11	—	—	150,000	8,200
12	320	22	160	455

TABLE IV.

PRESENCE OF BACILLUS COLI.

STATION.	SERIES I, OCT. 25.			SERIES II, NOV. 1.					
	In 1 c.c.	In $\frac{1}{10}$ c.c.	In $\frac{1}{100}$ c.c.	In					
1	−	−	−	1 c.c.	−	−	−	−	−
2	−	−	−	1 c.c.	−	+	−	−	−
3	−	−	−	1 c.c.	−	−	−	−	−
4	−	−	−	1 c.c.	+	−	+	−	−
5	−	+	−	1 c.c.	+	−	+	+	+
6	+	−	−	1 c.c.	+	+	+	+	+
7	+	−	−	1 c.c.	+	−	+	+	
8				$\frac{1}{10}$ c.c.	+	+	+	−	
9	+	−	−	$\frac{1}{10}$ c.c.	+	−	+	+	−
10	+	−	+	$\frac{1}{100}$ c.c.	−	−	−	−	−
11				$\frac{1}{100}$ c.c.	+	+	+	−	+
12	−	−	−	1 c.c.	−	−	−	−	−

Conclusions.

The first thing which attracts attention is the fact that, in a general way, the pollution was not so great with the first set of samples collected on October 25 as with the set of November 1; it is true, however, that this apparent difference is partly due to the absence, in the first set, of a sample from Station 11, where the pollution would naturally be expected to be at its maximum. Another striking feature is the absence of the factor which usually proves the most valuable indicator of pollution, the free ammonia. This is no doubt due to the peculiar nature of the contaminating material, one entirely different from house sewage, in that it decays only slowly. For this reason this particular case is in some ways better, in some ways worse, than might have been desired. On one hand, a factor usually relied upon for convicting evidence is removed; on the other, the changes taking place in such material are probably more prolonged than those which affect domestic sewage, and are therefore more readily followed and studied. Further, the Saxonville sewage did not very notably increase the chlorine content of the water, the nitrogen present as nitrites, or the organic matter oxidized by potassium permanganate. The diagrams plotted to show the results of these features of the chemical analysis furnish very little information (Figures 2 to 6). The residue on evaporation, the nitrogen present as albuminoid ammonia and nitrates, and the dissolved oxygen are, on the other hand, significant.

The albuminoid ammonia curves (Figure 3) show a rise on both days, with a maximum at the station nearest the mill. From this point the amount of albuminoid ammonia decreases, gradually, the last point at which an excess is noted being Station 7. The "suspended" albuminoid ammonia drops still more quickly, and shows a more regular change, a fact due, no doubt, to the direct and constant influence of sedimentation, assisted possibly by the filtering effect of weeds or sedges. The residue and hardness (Figures 2 and 5) show a somewhat similar relation, with the important difference that the fixed residue and hardness do not return quite to the normal in the whole length of the river, being perhaps added to by leakage of ground water along the course of the river. The very marked excess on the second day, however, disappeared below Station 8. The peculiar break in the residue curve at Station 6, on the first

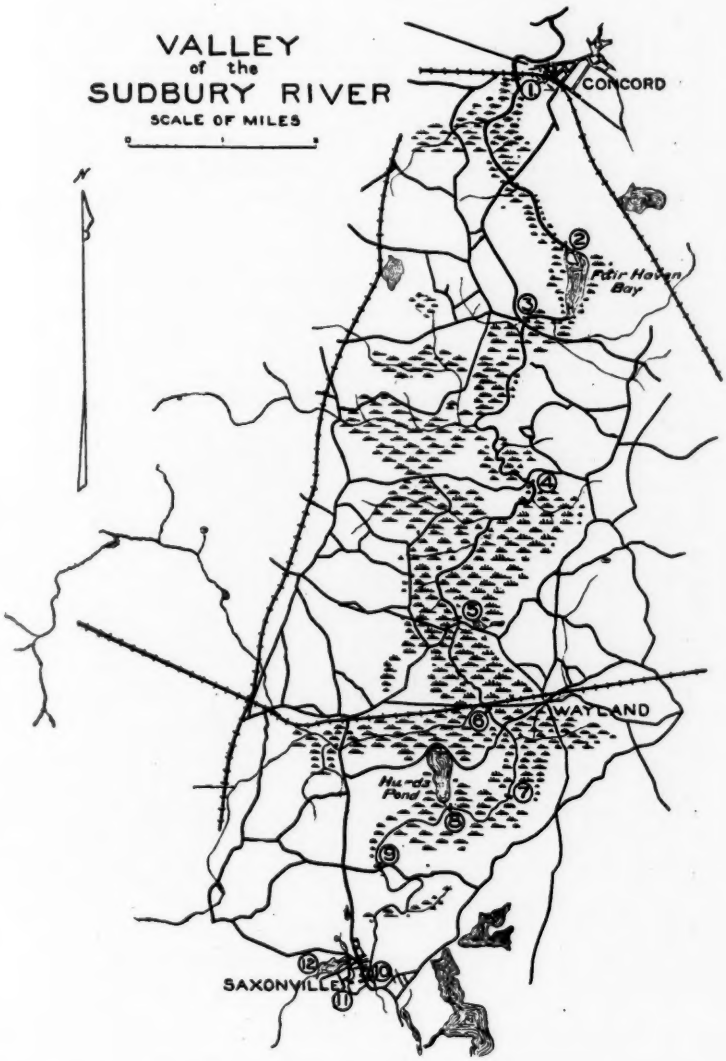

FIG. 1.—MAP OF THE SUDBURY RIVER BETWEEN SAXONVILLE AND CONCORD, SHOWING THE LOCATION OF THE STATIONS.

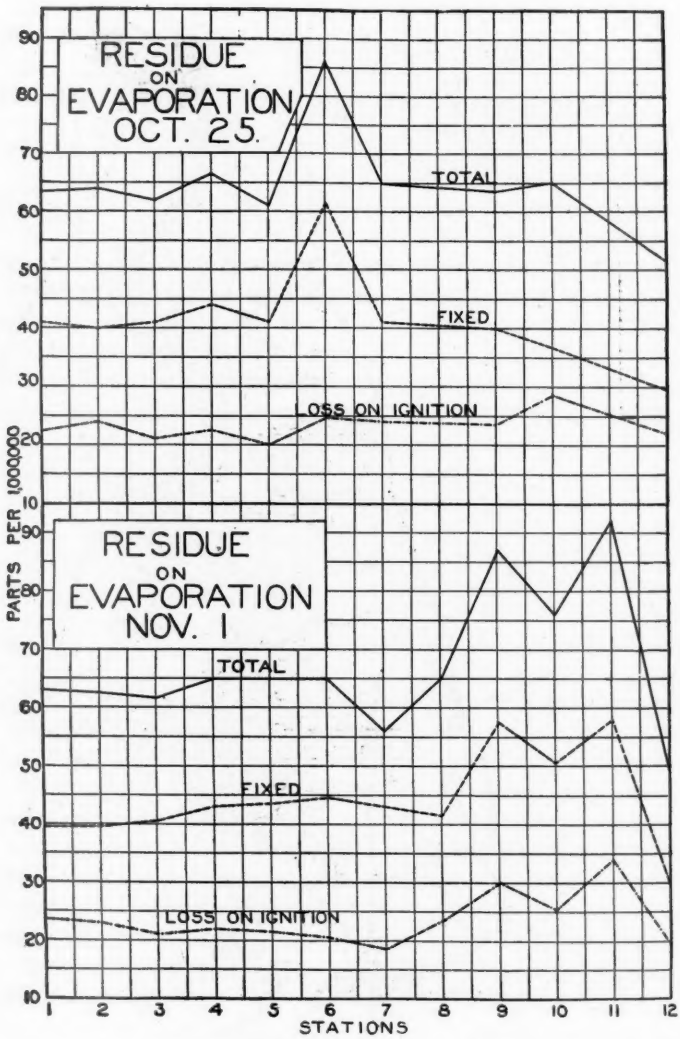

FIG. 2.—CHANGES IN CHEMICAL CONSTITUENTS IN PASSING FROM STATION 12, ABOVE SAXONVILLE MILLS, TO STATION 1, AT CONCORD. (To be read from right to left.)

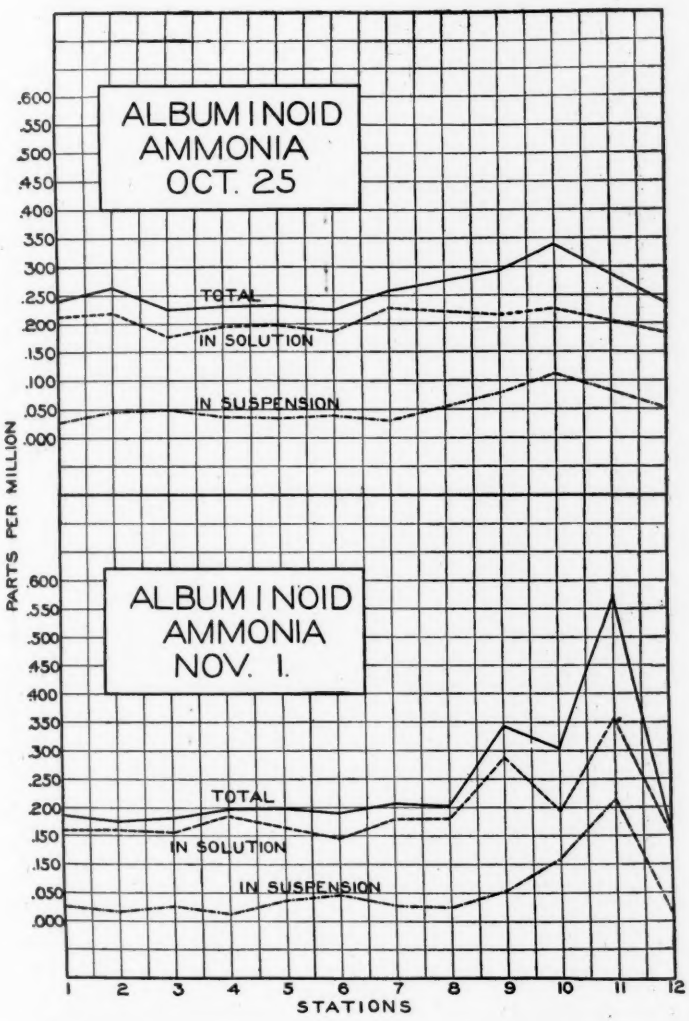

FIG. 3.—CHANGES IN CHEMICAL CONSTITUENTS IN PASSING FROM STATION 12, ABOVE SAXONVILLE MILLS, TO STATION 1, AT CONCORD. (To be read from right to left.)

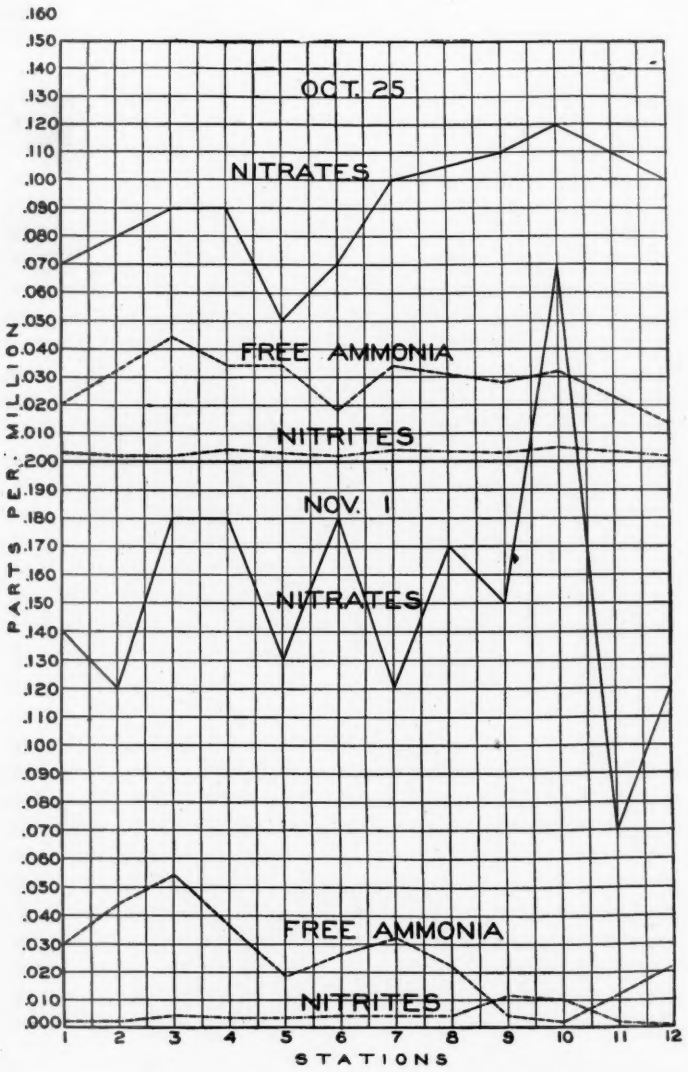

FIG. 4.—CHANGES IN CHEMICAL CONSTITUENTS IN PASSING FROM STATION 12, ABOVE SAXONVILLE MILLS, TO STATION 1, AT CONCORD. (To be read from right to left.)

day, may have been due to the admixture of a harder and clearer water from underground springs at this point. It will be noticed that it is accompanied by an increase in hardness and a decrease in color and oxygen consumed (Figure 5). The same thing is not observed in the second set. Possibly the sample at Station 6, on the second day, was not taken at exactly the same point as on the first day, but at a point a little above.

The irregular fluctuations of the nitrates (Figure 4) are somewhat hard to understand. The rise at Station 10, with a subsequent decrease, is evident in both curves; and the fall at Station 11, on the second day, is interesting as suggesting a reduction of the nitrates normally present in the water. The dissolved oxygen (Figure 6) shows the pollution and self-purification very clearly. A strong decrease on November 1, and a slight decrease on October 25, are followed by a gradual rise, reaching the normal between Stations 6 and 7; this region seems to be the crucial point of the chemical purification.

The results of the bacteriological analysis (Figures 7 and 8) are very instructive. The total number of bacteria present in the water, as determined by growth on gelatin at 20°, rose from one or two hundred to many thousand; a comparison of the figures for Stations 9 and 10 on the two dates showing, as do the chemical results, that the pollution was much greater on November 1 than on October 25. The fact that on both occasions Station 10 gave lower numbers than Station 9, may be due to the fact that a small spring feeds the river at the former place. The pollution, as measured by the bacteria, is evident as far down as Station 3, considerably below the point at which the chemical purification appears to be complete. The thermophiles, as measured by the growth on agar at 37°, show a similar enormous increase, but their disappearance in the river is more rapid, the numbers reaching about their normal values at Station 6, on the first day, and at Station 8 on the second. The examination of the samples for the colon bacillus, on the first day, showed that organism to be present in $\frac{1}{100}$ c.c. at Station 10, and in 1 c.c. below that point to Station 6. At Station 5 it happened that it was isolated from $\frac{1}{10}$ c.c., and not from the whole cubic centimeter examined; below that point it was not found. On the second day a certain dilution was chosen, and five duplicate samples examined. Bacillus coli was present four times out of five in $\frac{1}{100}$ c.c.,

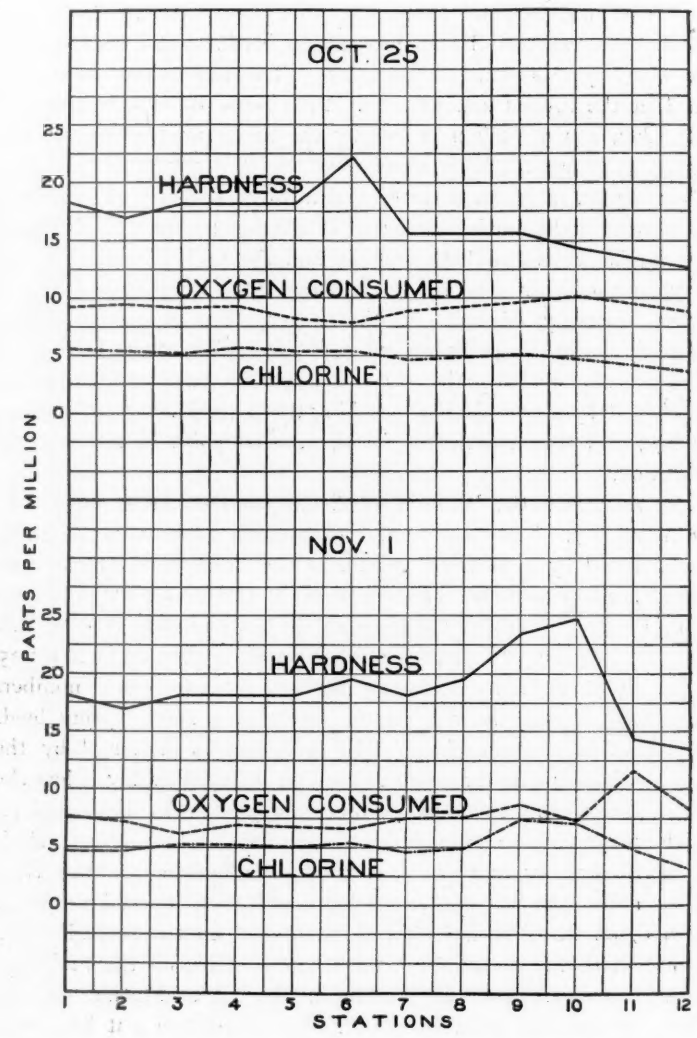

FIG. 5.— CHANGES IN CHEMICAL CONSTITUENTS IN PASSING FROM STATION 12, ABOVE SAXONVILLE MILLS, TO STATION 1, AT CONCORD. (To be read from right to left.)

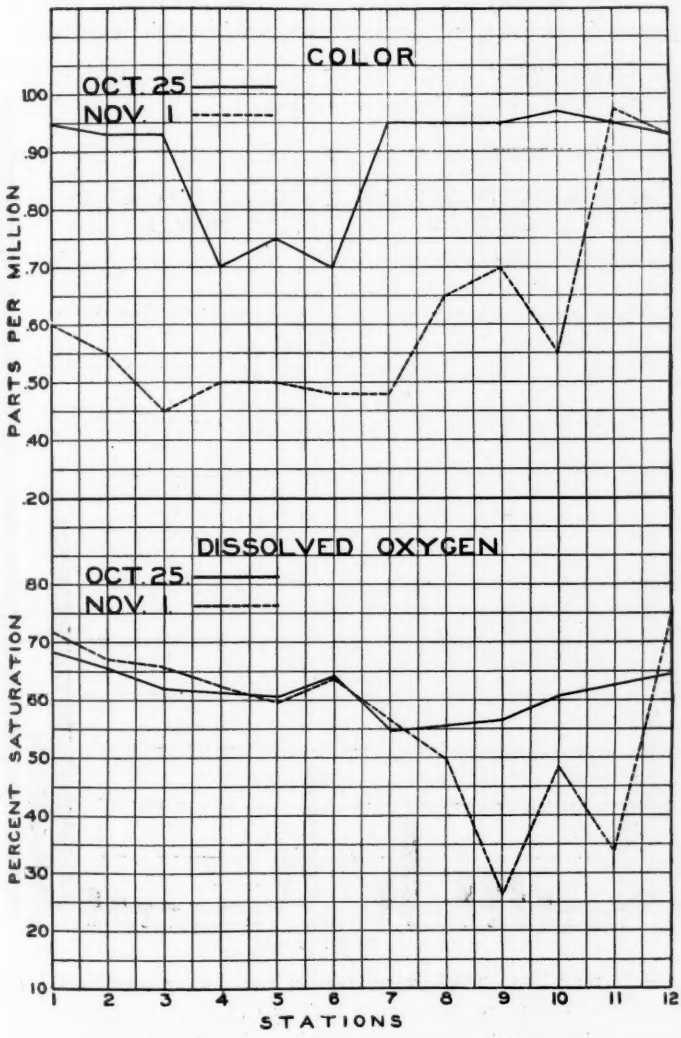

FIG. 6.—CHANGES IN CHEMICAL CONSTITUENTS IN PASSING FROM STATION 12, ABOVE SAXONVILLE MILLS, TO STATION 1, AT CONCORD. (To be read from right to left.)

at Station 11, but not at all in that quantity at Station 10. It was found in a majority of cases in $\frac{1}{10}$ c.c. at Stations 8 and 9. It appeared in a large majority of the single cubic centimeters ex-

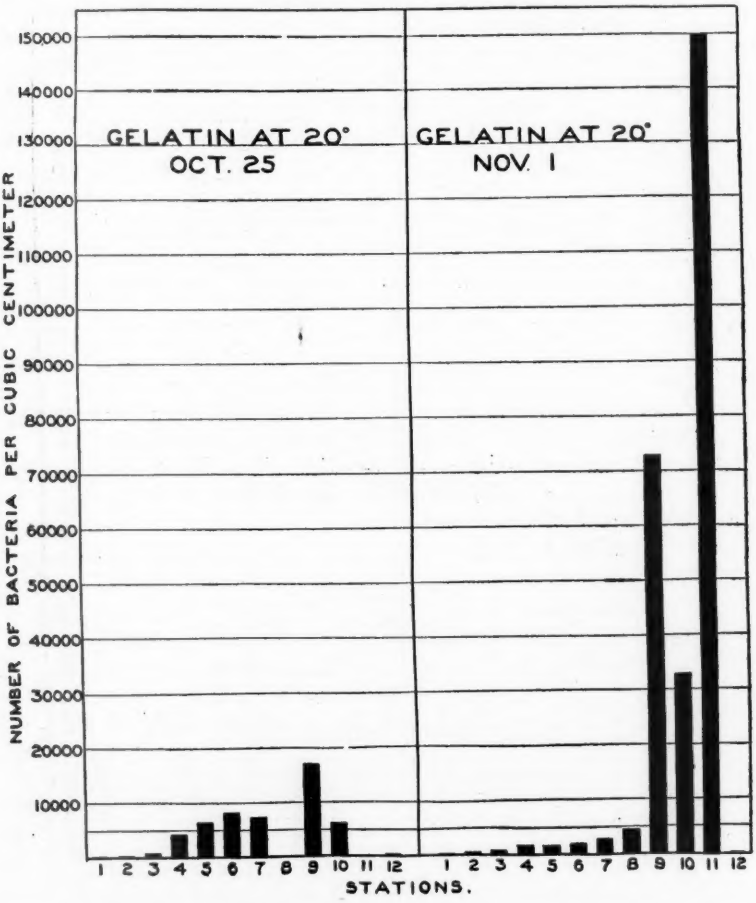

FIG. 7.— BACTERIA PER CUBIC CENTIMETER ON GELATIN AT 20° C. CHANGES IN BACTERIAL CONTENT IN PASSING FROM STATION 12, ABOVE SAXONVILLE MILLS, TO STATION 1, AT CONCORD. (To be read from right to left.)

amined from Stations 5, 6, and 7; in two cases out of five at Station 4, and in one case out of five at Station 2; not at all at Stations 1 and 3.

To summarize: The sewage of the Saxonville Mills, on the days observed, introduced into the Sudbury River a considerable amount of nitrogenous matter in the form of albuminoid ammonia, a consider-

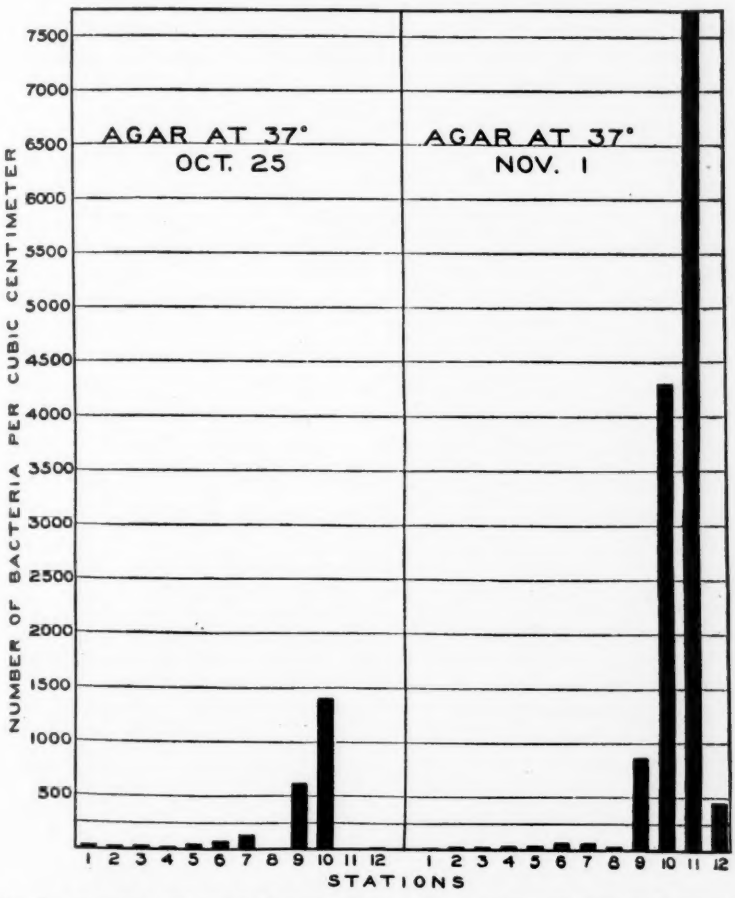

FIG. 8.— BACTERIA PER CUBIC CENTIMETER ON AGAR AT 37° C. CHANGES IN BACTERIAL CONTENT IN PASSING FROM STATION 12, ABOVE SAXONVILLE MILLS, TO STATION 1, AT CONCORD. (To be read from right to left.)

able amount of mineral matter appearing mainly as fixed residue and hardness, and great numbers of bacteria, many of them thermophiles with some intestinal bacilli. During the 6 miles between

Stations 11 and 7, organic pollution decreased and practically disappeared, at first rapidly, later more gradually. The thermophilous bacteria were removed during the same period. The total number of bacteria present was also greatly reduced, but remained distinctly above the normal during the sluggish flow of the stream to Station 4, 4 miles below. The colon bacillus also persisted in appreciable numbers down to Station 4.

It appears, then, that under certain conditions bacteria, and even intestinal bacteria, may persist in a stream after self-purification from organic material has been effected; and that they may be found, even in a very sluggish stream, at a distance of 10 miles below their point of entrance.

ON THE PHYSICAL PROPERTIES OF GELATIN, WITH REFERENCE TO ITS USE IN CULTURE MEDIA.

By GEORGE C. WHIPPLE.

Introductory.

It is a fact only too well known that "sterilized nutrient gelatin," as now prepared for use in bacteriological work, is a most unsatisfactory medium; and especially is it unsatisfactory for the quantitative determination of bacteria in water. If any illustration of this were needed, none better could be found than the recent report of the Committee on Standard Methods of Water Analysis of the Section of Bacteriology and Chemistry of the American Public Health Association.[1] In its Second Report of Progress this committee had recommended the following method of preparing nutrient gelatin, and by vote of the Section this method was adopted as the standard.

Standard Method for Making Nutrient Gelatin.

1. Infuse 500 g. lean meat 24 hours with 1,000 c.c. of distilled water in refrigerator.
2. Make up any loss by evaporation.
3. Strain infusion through cotton flannel.
4. Weigh filtered infusion.
5. Add 1 per cent. Witte's pepton and 10 per cent. Gold Label sheet gelatin.
6. Warm on water bath, stirring until pepton and gelatin are dissolved, and not allowing the temperature to rise above 60° C.
7. Neutralize.
8. Heat over boiling water (or steam) bath 30 minutes.
9. Restore loss by evaporation.
10. Titrate, after boiling one minute to expel carbonic acid.
11. Adjust reaction to + 1.0 per cent. by adding normal hydrochloric acid or sodium hydrate as required.
12. Boil two minutes over free flame, constantly stirring.
13. Make up loss by evaporation.
14. Filter through absorbent cotton and cotton flannel, passing the filtrate through the filter until clear.
15. Titrate and record the final reaction.
16. Tube, using 5 c.c. in each tube.

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[1] Advance Notes from the Third Report of Progress. Printed for the use of the Members of the Section.

17. Sterilize 15 minutes in the autoclav at 110°, or for 30 minutes in streaming steam on three successive days.

18. Store in the ice-chest in a moist atmosphere, to prevent evaporation.

In order to test the value of these directions in obtaining a medium of uniform quality and one which would give uniform results, the committee sent invitations to various bacteriologists, asking them to submit samples for comparative tests. Eighteen such samples were received. These were tested as to their reaction, melting-point, specific gravity, etc., and their relative value for determining the number of bacteria in a sample of water. The results of these tests are given in the following table:

TABLE I.

Sample number.	Sterilization.		Reported reaction.	Color; L=light; D=dark; V=very.	Specific gravity.	Melting-point. (Cent.)	Observed reaction.	Number of bacteria per c.c.	Per cent. of plates which were partially liquefied.	Per cent. of plates which were wholly liquefied.
	Temperature. (Cent.)	Time. (Minutes.)								
2	100	3 × 30	1.00	L	1.0324	33.0	1.63	125	0	0
1	115	10	1.00	L	1.0334	31.4	1.05	124	0	0
11	110	15	0 80	D	1.0265	27.1	1.17	112	0	0
7	113	20	1.00	L	1.0142	24 8	1.32	110	60	0
8	113	20	1.00	L	1.0208	27.1	1.12	108	0	0
3	110	15	1.00	L	1.0295	31.4	1.05	101	0	0
5	100	3 × 30	1.00	D	1.0250	21.5	1.53	98	20	0
12	100	3 × 30	*	L	1.0346	26.2	1.88	87	0	0
16	100	4 × 30	1.00	V L	1.0300	24.8	0.56	81	0	0
4	100	3 × —	1.00	D	1.0298	23.8	0.95	80	—	40
14	100	4 × 15	0.95	V L	1.0310	26.2	1.98	78	0	0
17	100	3 × 30	1.45	L	1.0344	23.8	1.52	72	60	0
10	110	2 × 15	1.10	D	1.0308	24.8	1.13	55	—	40
13	—	—	—	V L	1.0289	23.8	1.32	44	40	60
9	110	15	1.20	V D	1.0353	23.8	—1.70†	29	100	0
6	100	3 × 20	0.78	D	—	24.8	0.52	Liq.	—	100
15	—	—	1.00	D	1.0247	23.0	1.93	Liq.	—	100
18	100	3 × 15	1.10	L	1.0362	22.5	1 35	Liq.	—	100

* Reported as "too acid." † Probably abnormal.

It appeared from this comparison that the different media varied greatly in melting-point, in reaction, etc., and that their different physical and chemical qualities affected their value for quantitative work to such an extent that, when used to determine the number of bacteria in Brooklyn tap-water, the maximum number obtained was more than four times the minimum number. It appeared, moreover, that the melting-point of the gelatin, or the temperature at which it became soft, had the greatest effect upon the bacterial count. Thus, the average number of bacteria found upon the gelatins which had melting-points above 25° C. was 105 per c.c., while the average number found upon the gelatins which melted below 25° C. was 71 per c.c. This seemed to be due to the more rapid spreading of the liquefying colonies in the softer gelatins, with consequent ob-

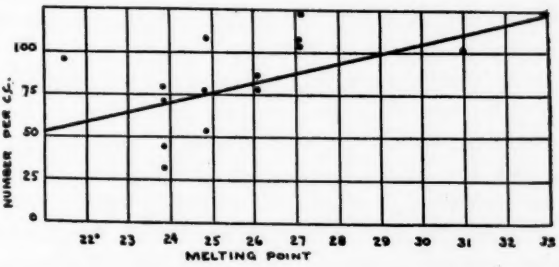

FIG. 1. — DIAGRAM SHOWING THE RELATION BETWEEN THE MELTING-POINT OF GELATIN AND THE BACTERIAL COUNT ON BROOKLYN WATER.

scuring of the smaller colonies. The general tendency for the firmer gelatins to give higher bacterial counts is shown in Figure 1. The unsatisfactory results shown by this comparison led the writer to undertake the present investigation.

NATURE OF GELATIN.

Gelatin is a substance of uncertain and varying composition obtained from the nitrogenous portions of bones, hides, connective tissues, etc. The collagen when boiled is changed to gelatin, which consists chiefly of glutin, derived from the skin and bones, with perhaps traces of chondrin, derived from the cartilage. Glue is practically impure gelatin plus a certain amount of some antiseptic and of some substance designed to increase its power of adhesion. There

are two principal kinds of gelatin — bone gelatin and hide gelatin. There is also isinglass, made from the bladders of sturgeon.

The general process of manufacture of hide gelatin is as follows:

The hide scraps are first macerated and subjected to the action of a solution of lime or caustic soda in pits for two or three weeks. This dissolves most of the blood and saponifies the fats. The excess of lime or soda is then largely removed by washing and the solution steamed to dissolve the gelatin, but an excess of heat is avoided. Sulphurous acid is used to bleach the gelatin. When of sufficient strength, the gelatin is allowed to harden in molds or on slabs, and is ultimately dried in sheets on wire nets. Bone gelatin is made in a somewhat similar manner. The bones are crushed, boiled, treated with hydrochloric acid, and the gelatin is ultimately dissolved as before, washed, bleached, and dried in sheets. The process requires a number of weeks.

When it is considered how great must be the variations in the character of the raw materials and how difficult it is to maintain constant conditions during the long process of manufacture, it is not surprising that the gelatins put upon the market should vary in quality; but the extent of these variations is far greater than bacteriologists have generally appreciated. It has been customary to specify "Gold Label Gelatin" as the standard; but practically this is of little value as a definition of quality. Some manufacturers make several grades of gold-label gelatin, and what is of even greater importance, the same brand of gelatin of any one dealer is not constant. It is seldom that two batches of gelatin are exactly alike both in their chemical and physical properties. When it is remembered how susceptible bacteria are to slight changes in environment, it is not to be wondered at that a culture medium which contains 10 per cent. of such a variable substance should at times give erratic results.

Gelatin is put upon the market in a variety of forms — thin sheets, thick sheets, broken sheets, shreds, granulated, and powdered. The sheet gelatin is almost invariably used by bacteriologists. The following is a list of the gelatins used in the experiments described beyond:

TABLE II. — List of Gelatins Used in the Experiments.

No.	Manufacturer.	Brand.	Obtained from.	Form.	Size of sheets. (Inches.)	No. of sheets to the pound.	Color.
1	....	"Gold Label"	Bausch & Lomb	Sheets	$3\frac{1}{4} \times 8\frac{1}{2}$	240	Light
2*	Heinrich	Hard Emulsion No. 9132	Paul Putmann	"	$3\frac{1}{4} \times 8\frac{1}{2}$	190	"
3	Compte Fils	"Gold Label"	Hoagland Laboratory	"	$2\frac{3}{4} \times 9$	135	"
4*	Nelson, Dale & Co.	Sheet Gelatin No. 3	Anthony & Co.	"	$3\frac{1}{4} \times 10$	28	Brown
5	Heinrich	"Gold Label"	Paul Putmann	"	3×9	290	Light
6	Compte Fils	"Gold Label"	Hibbert W. Hill	"	$2\frac{3}{4} \times 9$	135	"
7*	Cooper	A. A. Ex. Crescent	Factory	Broken sheets	..	.	Sl. dark
8*	Cooper	Crescent No. 1	"	"	..	..	Light
9	Coignet	Super Extra	Alfred Hodge & Co.	Sheets	3×9	16	Light yellow
10	....	Isinglass Russian	Eimer & Amend	Irregular sheets	..	..	Light
11	....	No. 1 Washed	....	..	..	..	"
12	....	Pure gelatin (prep.)		Amorphous	..	..	White
13*	Heinrich	Emulsion No. 1295	....	"	$3\frac{1}{4} \times 8\frac{1}{2}$	190	Light
14	Dentelle	Silver Label	Zinkeisen & Co.	Sheets	3×9	140	"
15	"	Neutral Gold Label	"	"	$3\frac{1}{4} \times 9\frac{1}{2}$	205	"

* Nos. 7 and 8 were glues rather than gelatins, and were not intended for bacteriological work. This fact should be noted in connection with Tables 14, 15 and 16, and with Plates XIII and XIV. Nos. 2, 4 and 13 were prepared especially for photographic purposes.

Physical Properties.

The physical properties of gelatin solutions may be described under the headings: *viscosity*, *spissitude*, and *melting-point*.

Viscosity.

When commercial gelatin is steeped in water it absorbs from five to ten times its weight of water, or enough to dissolve it at 30° C. In hot water gelatin solutions have a comparatively low viscosity, but as the temperature is lowered a "gelatinizing" process occurs, which increases the viscosity until the mass becomes a semi-solid.

The viscosity of gelatin solutions may be readily determined by

Tagliabue's viscosimeter,[1] designed to measure the viscosity of lubricating oils. This consists of a chamber which holds about 70 c.c., and which has at the bottom a standard aperture, controlled by a stop-cock. Through this the contents may be let out into a glass cylinder which holds 50 c.c., while the time required to fill the cylinder is determined with a stop-watch. The upper chamber is

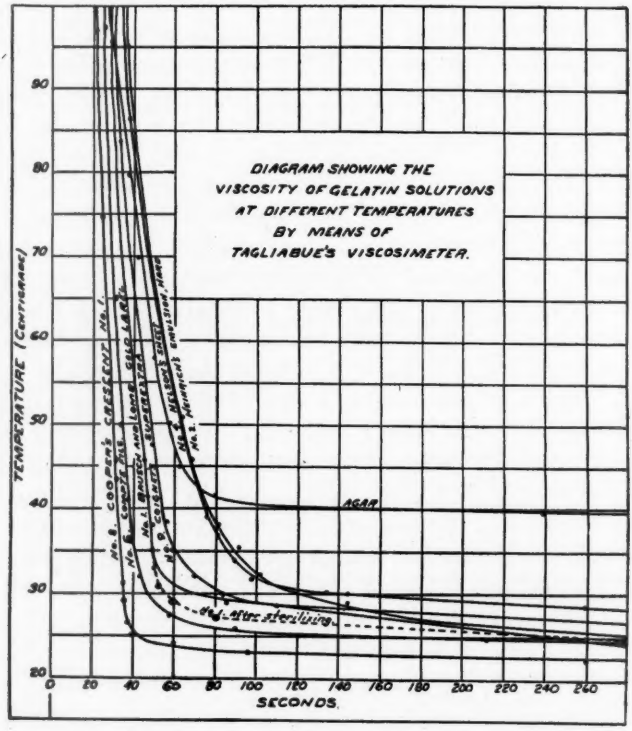

FIG. 2. — DIAGRAM SHOWING THE VISCOSITY OF GELATIN SOLUTIONS AT DIFFERENT TEMPERATURES.

water-jacketed, and there is a boiler to supply steam when it is desired to make the determinations at the boiling-point. A thermometer suspended in the liquid gives the temperature of the liquid as it flows out. With this apparatus the time required for

[1] See trade catalogue of Chas. J. Tagliabue, 51 Fulton St., New York. The instrument is described in most works on oil analysis.

50 c.c. of water at 100° C. to pass through the aperture is 20 seconds; at 20° it is about 24 seconds.

Figure 2 shows the viscosities of 10-per cent. solutions of various gelatins at different temperatures, from the boiling-point down to a temperature at which the gelatins would not flow through the aperture. It will be observed that at 100° C. the time required for 50 c.c. of the gelatin solutions to flow through the aperture varied from 22 to 35 seconds, the average being about 30 seconds. In some cases the solution maintained a comparatively low viscosity until it began to set, or gelatinize, while in other cases the increase of viscosity with falling temperatures was greater, and the point at which setting occurred was approached more gradually. The average viscosity of the gelatins tested at 40° C. was about 55 seconds — which is not far from double the average at 100° C. The viscosities at 100° and 40° C. seem to bear but little relation to the melting-points or the spissitudes of the solutions after setting has occurred. When, however, a gelatin solution has its gelatinizing power impaired by heating, its viscosity at low temperatures is reduced, as shown by the dotted line in Figure 2.

The viscosity of gelatin solutions affects the bacteriologist chiefly in volumetric measurement. A pipette, for example, will discharge a larger amount of a solution at 100° C. than at 40°, because there will be less adhesion of the solution to the glass walls in the case of the warmer liquid. For example, a pipette which discharged 5 grams of gelatin at 98° C. was found to discharge but 4.92 grams at 30° C. This error is usually too small to be taken into account.

Spissitude of Gelatin Solutions.

By the spissitude of a gelatin solution is meant its consistency, or degree of stiffness, or its "jelly-strength," as it is sometimes called. An apparatus used to measure this may be termed a spissimeter. Various forms of apparatus for this purpose are said to be used by manufacturers of glue and gelatin, but these are generally held as trade secrets. In Wagner's *Chemical Technology* there is a description of an apparatus proposed by Lipowitz (see Figure 3). This consisted of a "saucer-like" piece of tinned iron fastened with its convex side downwards at the lower end of a wire. This saucer was allowed to rest upon the surface of the gelatin to be tested, the wire

extending vertically upwards through guides in an iron framework, and terminating at the top in a funnel into which shot could be poured to increase its weight. The iron frame rested upon the sides of the receptacle which contained the gelatin. The weight required to force the saucer downwards into the gelatin, when allowed to stand in 10-per cent. solution for 12 hours at 18° C., was taken as a measure of the spissitude, the greater the consistency of the gelatin the more weight being required.

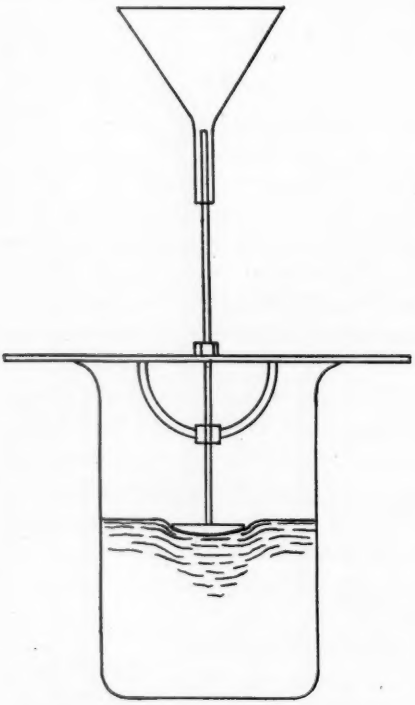

FIG. 3.—SPISSIMETER AFTER LIPOWITZ.

Elaborate methods are in daily use for measuring the viscosity of asphalt,[1] and with suitable modifications these might serve for measuring the spissitude of gelatin solutions. The writer has found, however, that a simpler device, namely, Vicat's[2] apparatus, designed for determining the time of setting of cements, gives excellent results.

VICAT'S APPARATUS USED AS A SPISSIMETER.

This apparatus is shown in Figure 4. It consists of a brass plunger mounted so as to slide vertically in the guides of an iron casting, and provided with an index which moves over a graduated scale screwed to the casting and which serves as a means for measuring the amount of motion. A platform at the top of the plunger offers opportunity for using additional weights, but these are seldom

[1] One of these is described in the Report of the Inspector of Asphalt and Cements, U. S. District of Columbia, for the year ending June 30, 1901.

[2] This is described in almost all works on Cement Testing.

needed, as the plunger without the platform is usually sufficient. A set screw on the side serves to stop movement when desired. The gelatin to be tested is placed in a wide-mouthed bottle, or some other convenient receptacle, and the plunger is lowered until contact is made with the surface of the gelatin, at which point the position of the index on the scale is read. The set screw is then gently loosened, and the plunger allowed to rest its weight upon the gel-

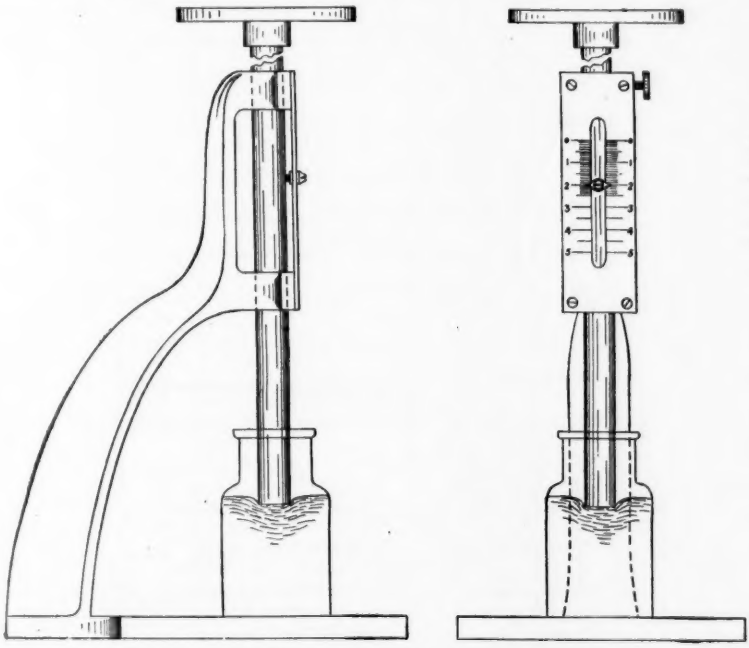

FIG. 4. — VICAT'S APPARATUS USED AS A SPISSIMETER.

atin for about 5 seconds, after which the set screw is tightened and a second reading made. The difference between the two readings gives the penetration of the plunger in millimeters, and this serves as a measure of the spissitude. The scale may be read to 0.1 mm., and except in the case of high readings this figure represents practically the limit of error.

Preliminary experiments were made with aluminum plungers set in wooden rods. Six of these were used, with diameters varying from 3 to 12½ mm., and with loads varying from 25 to 1,000 grams. They

were used with both square and round ends. The results obtained upon a 10-per cent. solution of gelatin which had been kept for 24 hours at a temperature of 20° C., are shown in Figure 5. It was found that the amount of penetration increased gradually with the

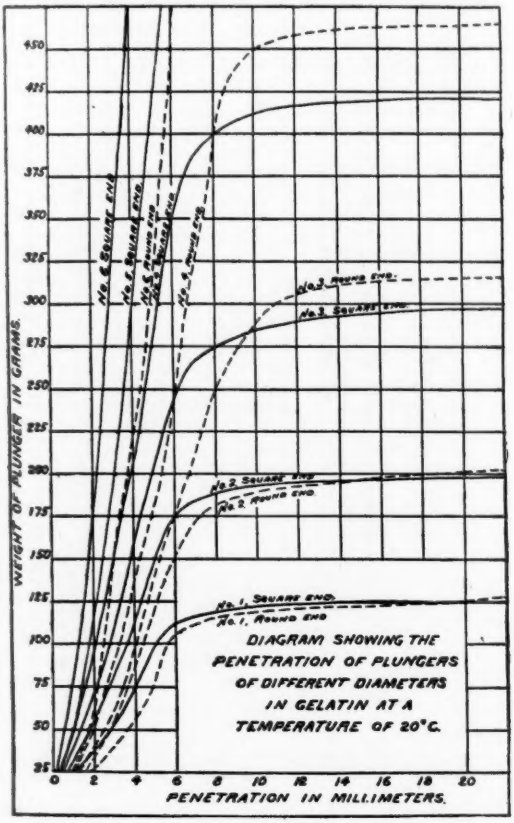
WEIGHT OF PLUNGER IN GRAMS.
PENETRATION IN MILLIMETERS.
No. 6 Square End.
No. 6 Round End.
No. 5 Square End.
No. 5 Round End.
No. 4 Square End.
No. 4 Round End.
No. 3 Round End.
No. 3 Square End.
No. 2 Square End.
No. 2 Round End.
No. 1. Square End.
No. 1. Round End.
DIAGRAM SHOWING THE PENETRATION OF PLUNGERS OF DIFFERENT DIAMETERS IN GELATIN AT A TEMPERATURE OF 20°C.

Fig. 5.

load until a certain point had been reached, after which the plunger rapidly passed downward into the gelatin. The weight required to do this varied with the different plungers as follows:

TABLE III.

Number of plunger.	Diameter in millimeters.	Area of cross-section in square millimeters.	End of plunger.	Weight required to produce penetration of 4 mm. (in grams).	Weight required to make plunger sink in gelatin (in grams).
1	3	7.06	Square.	75	125
1	3	7.06	Round.	56	125
2	4.5	15.89	Square.	110	200
2	4.5	15.89	Round.	85	200
3	6	28.26	Square.	157	300
3	6	28.26	Round.	96	325
4	7.5	44.16	Square.	205	425
4	7.5	44.16	Round.	135	475
5	10	78.54	Square.	295	....
5	10	78.54	Round.	....	....
6	12.5	122.41	Square.	475	900
6	12.5	122.41	Round.	220	1,500

It was found that if the weight required to make the plunger sink in the gelatin was taken as a measure of the spissitude, it was preferable to have the lower end rounded; but if the penetration was taken as the measure, the square end was better.

When the square-ended plunger was used, it was found that the weight required to produce a certain penetration of the plunger and the weight required to make the plunger sink in the gelatin increased regularly with the area of the cross section of the plunger. Of the various plungers, that which had a diameter of 10 mm. was selected as giving the best results with the strengths of gelatin solution likely to be used. This, furthermore, was the diameter of the plunger furnished with the Vicat apparatus. Lipowitz's method of observing the weight necessary to cause the plunger to sink was discarded, because the weights required to produce this result were not, it was found, directly proportional to the strengths of the gelatin solutions tested. It was also lacking in sensitiveness and in precision.

The method of observing the penetration of the plunger was found to be much more satisfactory.

The plunger selected for use had a diameter of 10 mm. and weighed 150 grams. It was allowed to rest for five seconds on the surface of the gelatin solution, which was of course parallel to the flat head of the plunger, after which the depth of penetration was read from the scale. By using this apparatus upon gelatin solutions of different percentage strength, it was found that the strength of the solution varied directly with the reciprocal of the depth of penetra-

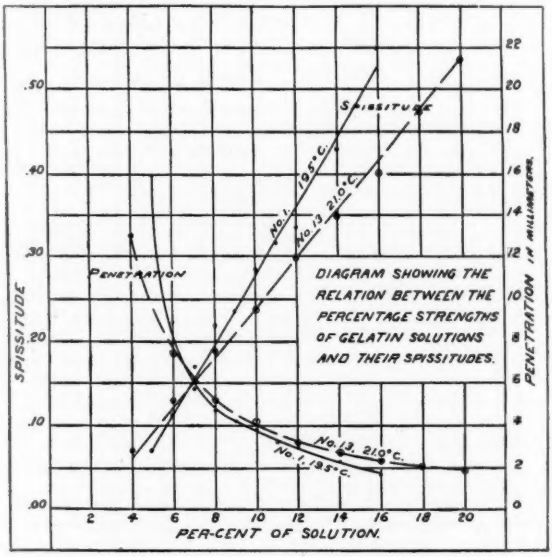
SPISSITUDE
PENETRATION IN MILLIMETERS.
PER-CENT OF SOLUTION.
PENETRATION
No. 1, 19.5° C.
No. 13, 21.0° C.
DIAGRAM SHOWING THE RELATION BETWEEN THE PERCENTAGE STRENGTHS OF GELATIN SOLUTIONS AND THEIR SPISSITUDES.

FIG. 6.

tion in millimeters. This may be seen from Figure 6, which shows, by the curves, the depths of penetration after the gelatin solutions had been allowed to stand for twenty-four hours at a temperature of 20° C. Their reciprocals, when plotted, fall approximately in straight lines, and thus furnish a direct measure of spissitude. *The spissitude of a gelatin solution,* therefore, as determined by the method here suggested, *may be defined as the reciprocal of the depth in millimeters to which a plunger having a diameter of 10 mm. and a weight of 150 grams will penetrate when allowed to bear upon the surface of the solution for five seconds. Different gelatins are preferably*

compared in 10-per cent. solutions after they have been allowed to set for twenty-four hours at 20° C. (This strength of solution and temperature are those generally used in bacteriological work.)

In observing the spissitude, it should be noted that if too small a receptacle is used for the gelatin solution, the depth to which the plunger will sink will be somewhat influenced by it. This is shown by the following figures:

TABLE IV.

Diameter of Receptacle in Millimeters.	Depth of Penetration.
95	4.9
60	4.8
45	4.6
15	1.2

With a certain gelatin which showed a penetration of 6 mm. the diameter of the circle of depression was found to be about 30 mm., the plunger being 10 mm. in diameter. With low spissitudes, therefore, it is desirable to use a receptacle somewhat larger than the bottle shown in Figure 4.

Factors which Affect the Spissitude of Gelatin Solutions.

Character of the Gelatin.

Commercial gelatins differ considerably in their jelly-strength, that is, in the percentage of gelatinizing substances present. This is evident from the following table of spissitudes of 10-per cent. solutions determined under the standard conditions mentioned above.

TABLE V.

Number of Sample.	Celatin.	Spissitude.
1	Bausch & Lomb's Gold Label.	.24
2	Heinrich's Hard Emulsion.	.15
3	Compte Fils' Gold Label.	.11
4	Nelson's Sheet, No. 3.	.23
5	Heinrich's Gold Label.	.34
6	Compte Fils' Gold Label.	.09
7	Cooper's AA Ex. Crescent.	.20
8	Cooper's, No. 1.	.06
9	Coignet's Superextra.	.22
10	Russian Isinglass.	.26
13	Heinrich's Hard Emulsion.	.20
14	Dentelle's Silver Label.	.13

Strength of Solution.

The strength of the solution is naturally of fundamental importance. With a constant temperature, the spissitude varies directly with the percentage strength, as shown by Figure 6. With most commercial gelatins a 1-per cent. solution will gelatinize, and in some cases even a weaker solution will do so if the temperature is low enough; but ordinarily the spissitude at 20° C. of solutions less than 4-per cent. strength is too low to be observed with the spissimeter.

The ratio between spissitude and percentage strength of solution varies, however, with the temperature, as shown by Figure 7, higher temperatures showing less increase in spissitude with increasing percentage strengths than is the case with low temperatures.

Temperature.

Temperature has a most important effect upon the spissitude of gelatin solutions, and with some latitude it may be said that the spissitude varies inversely with the temperature. For example, a 10-per cent. solution of gelatin may have a spissitude of 0.70 at 10° C.; when this is warmed to 20° the spissitude may be .25; at 25° C. it may be .12; and at 30° C. the solution may have become almost, if not quite,

fluid. Different gelatins do not all behave in the same way when heated. Some become soft more rapidly than others. This is illustrated in Figure 8.

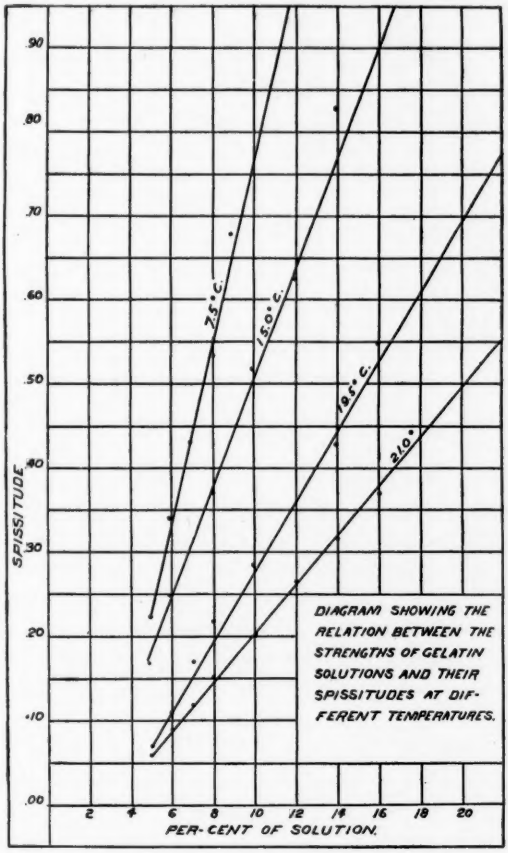

FIG. 7.

Time of Setting.

The setting of gelatin solutions is a gradual process. In the observations of viscosity it was seen that the solution gradually became thicker. The same gelatinizing process goes on after the solution has apparently set. This is seen in Figure 9, which shows the

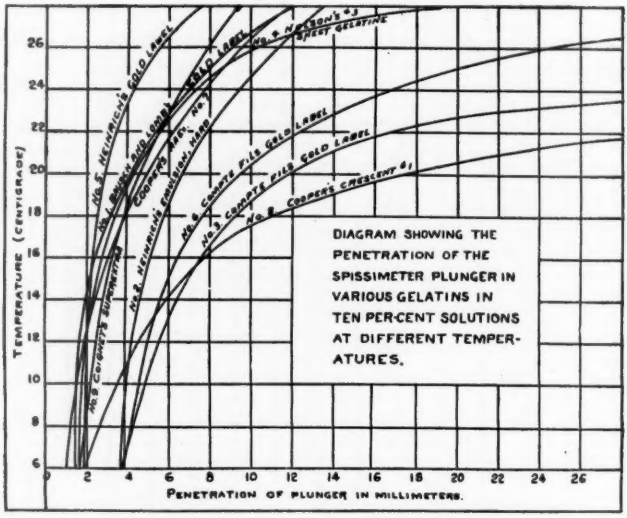
DIAGRAM SHOWING THE PENETRATION OF THE SPISSIMETER PLUNGER IN VARIOUS GELATINS IN TEN PER-CENT SOLUTIONS AT DIFFERENT TEMPERATURES.
TEMPERATURE (CENTIGRADE)
PENETRATION OF PLUNGER IN MILLIMETERS.
No 2 NELSON'S #3 SILVER GELATINE
No 2 COOPER'S CRESCENT #1
No 2 COOPER'S AMER. #1
No 2 COOPER'S EMPIRE #1
No 2 COOPER'S SUPERFINO
No 2 COMPTE FILS GOLD LABEL
No 3 COMPTE FILS GOLD LABEL

FIG. 8. — EFFECT OF TEMPERATURE UPON SPISSITUDE.

gradual increase in the spissitude of a 10-per cent. gelatin solution, which was maintained at a temperature of 20° for five days. It is said that some gelatins require even a longer time for the spissitude to become constant. Gelatin solutions set more rapidly when cold than when warm. Hence, in comparing the spissitude of different gelatins it is important not only that the observation be made at a definite temperature, but that a constant temperature be maintained while the gelatin is setting. It should not be forgotten, moreover, that when

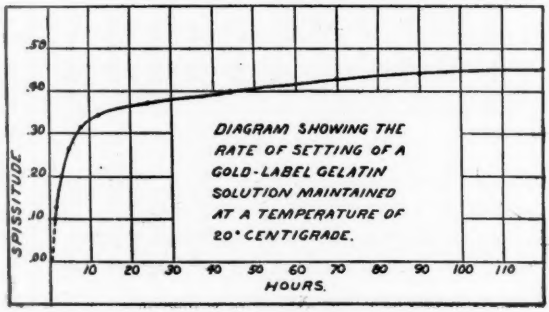
DIAGRAM SHOWING THE RATE OF SETTING OF A GOLD-LABEL GELATIN SOLUTION MAINTAINED AT A TEMPERATURE OF 20° CENTIGRADE.
SPISSITUDE
HOURS.

FIG. 9. — TIME OF SETTING.

gelatin solutions are allowed to stand there is an evaporation from the surface, with the consequent formation of a thin skin of gelatin upon the surface of the medium. This evaporation should be guarded against by tightly closing the receptacle or by keeping it in an atmosphere saturated with moisture.

Effect of Heat.

When gelatin solutions are heated, they gradually lose their gelatinizing power. This is of great importance to the bacteriologist, inasmuch as sterilization by heat is an essential part of the preparation of culture media. At the present time two methods of steriliza-

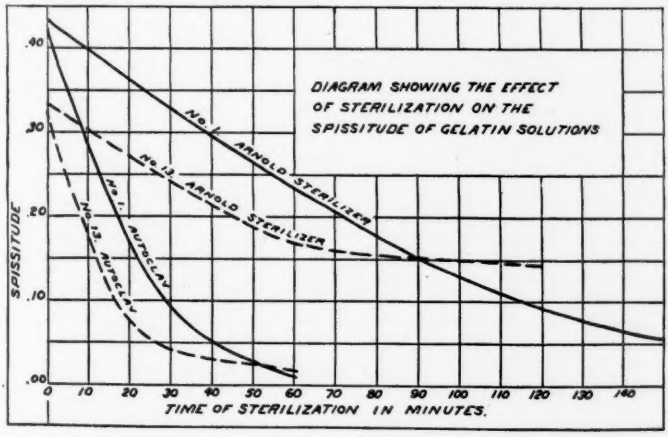
DIAGRAM SHOWING THE EFFECT OF STERILIZATION ON THE SPISSITUDE OF GELATIN SOLUTIONS
No. 1. ARNOLD STERILIZER
No. 13. ARNOLD STERILIZER
No. 1. AUTOCLAVE
No. 13. AUTOCLAVE
SPISSITUDE
.40
.30
.20
.10
.00
0 10 20 30 40 50 60 70 80 90 100 110 120 130 140
TIME OF STERILIZATION IN MINUTES.

FIG. 10. — EFFECTS OF HEATING FOR VARIOUS LENGTHS OF TIME.

tion are used indiscriminately: namely, heating in streaming steam for fifteen to thirty minutes on two or three successive days, and heating once for ten to twenty minutes in steam under a pressure of ten to fifteen pounds per square inch. Inasmuch as both the temperature and time of exposure affect the spissitude, it is evident that present practice is lacking in exactness. Figure 10 shows the decrease in spissitude of two gelatins in 10-per cent. solution when heated for various lengths of time in an Arnold Steam Sterilizer and in an autoclave. The Arnold sterilizer required about one minute to get up steam after the gelatin was put in. The autoclave required about fifteen minutes to reach a pressure of fifteen pounds. The safety valve was then allowed to blow off, thus letting out the air,

and the time of sterilization was counted from this point. It usually took from two to four minutes for the pressure to return to fifteen pounds. After sterilization the pressure was at once relieved by opening the valve, and the gelatin was removed as soon as possible. The gelatin was tested in two-ounce bottles, like that shown in Figure 4.

It will be noticed that for equal periods of time sterilization in the autoclave has a much greater effect upon the spissitude than sterilization in streaming steam. Ordinarily the time required for sterilization in the autoclave is from ten to twenty minutes (plus the time required to get up steam, which is not usually taken into account), while with the Arnold sterilizer sixty to ninety minutes are usually allowed. If the resulting spissitudes are compared on this basis, it will be seen that the autoclave has a less deleterious effect on the gelatinizing power of the solution than does the Arnold sterilizer. This is in accord with the results obtained by the Committee on Standard Methods of Water Analysis [1] in their comparison of culture gelatins. It was observed that gelatins sterilized in the autoclave had generally higher melting-points than those sterilized in the Arnold sterilizer. Inasmuch as different gelatins behave somewhat differently when heated, it is not possible to find a definite ratio between the two methods of sterilization; but approximately one and one-half hours' sterilization in streaming steam has the same effect on the spissitude as twenty minutes in steam under fifteen pounds' pressure, and one hour's sterilization in steam has the same effect as fifteen minutes under pressure. Discontinuous sterilization modifies somewhat the effect of sterilization in steam. A gelatin steamed for one and one-half hours continuously will have a lower spissitude than the same gelatin allowed to cool on the ice between each half-hour's heating. Manifestly the preliminary heating in the autoclave has an important effect and should be reduced to the lowest possible time.

In establishing a standard procedure for sterilization, it is preferable to decide upon three sterilizations of thirty minutes each in streaming steam, and to allow such time in the autoclave as would produce an equal spissitude of the gelatin solution. This can be easily obtained by experiment for any particular gelatin.

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[1] Loc. cit.

The bulk of the gelatin solution when sterilized has a very important effect on the resulting spissitude. It makes a great difference whether gelatin is sterilized in tubes or in flasks containing, say, 500 c.c. Thus a 10-per cent. solution of Bausch & Lomb's gelatin was sterilized for ten minutes at 115° C. in flasks of 500 c.c. and in tubes of 5 c.c., with the following results:

TABLE VI.

Gelatin.	Spissitude.	Melting-point.
Before sterilization	.22	30.7° C.
500 c.c. in flasks, cooled in the ice chest	.17	28.8° C.
5 c.c. in test tubes, cooled in the ice chest	.06	28.2° C.
500 c.c. in flasks, cooled at room temperature	.13	28.6° C.
5 c.c. in test tubes, cooled at room temperature	.04	27.8° C.

The longer time required for the heat to penetrate the mass of gelatin in the flask is shown in the smaller reduction of its spissitude. The beneficial effect of rapid cooling of the gelatin after sterilization is also shown.

Spissitude of Agar.

Solutions of agar are much firmer than those of gelatin, as shown by the following table; but the solution is far less elastic, and the circle of depression around the plunger is very small. A 1-per cent. solution gives a depression of the plunger under standard conditions of 3.5 mm. (= spissitude .29), while an 0.8-per cent. solution offers so little resistance to the plunger that it readily sinks to the bottom of the glass. Used as a glue, agar has almost no value; but a small amount of agar added to a solution of gelatin increases its spissitude considerably.

TABLE VII.—SPISSITUDE OF AGAR.

Per cent. of Solution.	20° C.	37° C.
0.8	....	....
1.0	.29	....
1.2	.36	.29
1.4	.45	.31
1.6	.50	.36
1.8	.56	.50
2.0	.67	.55

Melting-point.

When a solution of gelatin which has become hard is gently warmed, the mass will first become soft and then fluid. The exact temperature at which the mass becomes fluid is sometimes a difficult matter to determine, inasmuch as a certain time is required for the change to take place. For example, a gelatin solution may remain hard for five minutes at a temperature of 27° C., but if kept for several hours at that temperature it may melt. Then the strength of the solution affects the melting-point to some extent. This is shown by the following figures:

TABLE VIII.

Kind of Gelatin.	Per cent. of Gelatin in Solution.	Melting-point (two minutes' exposure).	Melting-point (five minutes' exposure).
No. 13	1	29.0° C.	29.0° C.
"	2	32.0° C.	32.0° C.
"	4	35.0° C.	33.0° C.
"	6	36.0° C.	35.0° C.
"	8	36.0° C.	35.0° C.
"	10	36.0° C.	36.0° C.
"	12	36.0° C.	36.0° C.
"	14	36.0° C.	36.0° C.
"	16	36.0° C.	36.0° C.
"	18	37.0° C.	37.0° C.
"	20	37.0° C.	37.0° C.

The method of comparison here used was to take 5 c.c. of a 10-per cent. solution in a thin-walled $\frac{5}{8}$-inch test tube and slant the surface as is done for agar cultures; then to immerse this in water and raise the temperature one degree at a time, at intervals of five minutes. The temperature at which the gelatin softened, so that its surface became level, was considered as the melting-point.[1]

The melting-points of the gelatins tested in this manner were as follows:

TABLE IX.

Number of Sample.	Gelatin.	Melting-point.	Congealing-point.
1	Bausch & Lomb's Gold Label.	30.7° C.	23.0° C.
2	Heinrich's Hard Emulsion, No. 9132.	31.5° C.	23.0° C.
3	Compte Fils' Gold Label.	28 5° C.	21.0° C.
4	Nelson's Sheet, No. 3.	28.5° C.	21.0° C.
5	Heinrich's Gold Label.	33.0° C.	22.0° C.
6	Compte Fils' Gold Label.	28.5° C.	21 0° C.
7	Cooper's AA Ex. Crescent.	29.6° C.	22.0° C.
8	Cooper's, No. 1.	26.8° C.	20.0° C.
9	Coignet's Superextra.	28.0° C.	22.0° C.
10	Russian Isinglass.	24.5° C.	18.0° C.
13	Heinrich's Hard Emulsion, No. 1395.	34.0° C.	23.0° C.
14	Dentelle's Silver Label.	28.0° C.	20.0° C.

The melting-point of a gelatin solution bears a more or less direct relation to its spissitude. This is illustrated by Figure 11.

The melting-point of a gelatin solution is lowered by heating. This is a well-known fact, and is sufficiently illustrated by Figure 12.

The temperature at which a gelatin solution will become hard is usually a number of degrees below its melting-point. Time and strength of solution affect this determination also; but the point may be obtained with sufficient accuracy by following the method used for obtaining the melting-point, allowing the bath to cool one degree at

[1] This criterion was suggested by Mr. R. B. Fitz-Randolph, of the Hoagland Laboratory, Brooklyn, N. Y.

five-minute intervals, and noting the temperature at which the gelatin, kept in a slanting position, ceases to flow when the tubes are held erect. The congealing-points of the gelatins experimented with are given in Table IX.

By the addition of various substances to gelatin solutions the spissitude may be raised or lowered, but a discussion of this subject would carry us out of bounds. The substances used in culture media have but little effect on the spissitude. Thus 5 per cent. of pepton reduced the spissitude of a 10-per cent. gelatin solution from 0.21

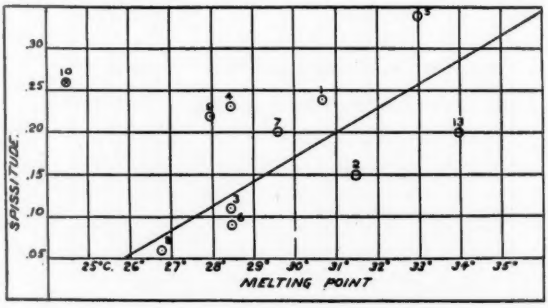

FIG. 11. — DIAGRAM SHOWING THE RELATION BETWEEN MELTING-POINTS AND SPISSITUDES OF GELATIN SOLUTIONS.

to 0.19, while 1 per cent. of salt made an equal reduction. Larger amounts of salt, however, produce greater reductions. Thus:

TABLE X.

Per cent. of Salt in Solution.	Spissitude of a 10-Per cent. Gelatin Solution.
0.0	.26
0.5	.25
1.0	.24
2.0	.23
3.0	.20
5.0	.17
10.0	.10

The reaction of the solution within the limits used in culture media has also no appreciable effect, but extremes in either direction appear to reduce the spissitude. Thus:

TABLE XI.

Reaction of Solution.	Spissitude.
−10.0	.11
−4.0	.26
−2.0	.32
−1.0	.31
0.0	.31
+1.0	.31
+2.0	.33
+4.0	.26
+10.0	.18

Absorption of Water.

Sheet gelatin when kept in a moderately dry atmosphere contains from 3 to 8 per cent. of water. When kept in a moist atmosphere, however, it rapidly absorbs water and becomes soft. The following experiment illustrates this: — Ten grams of thin sheet gelatin (Sample No. 1) were dried to constant weight and then put in a 20° incubator, which had its atmosphere saturated with moisture. After twenty-four hours it had absorbed 18 per cent. of water; after forty-eight hours, 28 per cent.; after ninety-six hours, 32 per cent.; and after two weeks, 45 per cent. It is evident that if the gelatin contains a considerable amount of moisture, this will have an effect upon the strength of the gelatin in the culture medium. Gelatins, therefore, should be kept in a dry place, or else heated before using. Incidentally it may be said that heating the dry gelatin in sheets has no effect on its spissitude.

Reaction.

Most gelatins sold are said to be "free of acid," but when tested by the methods used by bacteriologists this is seldom found to be true. The reactions of the gelatins tested were as follows:

TABLE XII.

Number.	Gelatin.	Reaction.	
		With Phenolphthalien.	With Rosolic Acid.
1	Bausch & Lomb's Gold Label	+ 1.8%	+ 0.6%
2	Heinrich's Hard Emulsion, No. 9132.	+1.2%	+ 0.5%
3	Compte Fils' Gold Label.	+ 2.2%	+ 1.2%
4	Nelson's Sheet, No. 3.	+ 0.4%	— 1.9%
5	Heinrich's Gold Label.	+ 2.2%	+ 1 8%
6	Compte Fils' Gold Label.	+ 2.3%	+ 1.3%
7	Cooper's AA Ex. Crescent.	+ 1.7%	+ 0.5%
8	Cooper's No. 1.	+ 1.7%	+ 0.7%
9	Coignet's Superextra.	+ 2.1%	+ 1.0%
10	Russian Isinglass.	+ 1 5%	+ 0.3%
11	No. 1. Washed.	+ 1.3%	+ 0.4%
13	Heinrich's Hard Emulsion, No. 1395.	+ 1.2%	+ 0.5%
14	Dentelle's Silver Label.	+ 2.0%	+ 1.0%
15	Neutral Gelatin from Zinkeisen & Co	+ 1.8%	....

It was found that the reaction was not materially altered by boiling.

Effect of the Character of the Gelatin on the Quantitative Bacteriological Examination of Water.

Sedgwick and Prescott[1] in 1895 showed that differences in the character of the gelatin affected the quantitative bacteriological exam-

[1] On the Influence of Variations in the Composition of Nutrient Gelatin upon the Development of Water Bacteria, by Wm. T. Sedgwick and S. C. Prescott. Journal American Public Health Association, October, 1895.

ination of water. They compared culture media made with bone gelatin and hide gelatin with those made with the "ordinary gelatin used in bacteriological work." Their results were as follows:

TABLE XIII. (FROM SEDGWICK AND PRESCOTT.)

Sample.	Number of Bacteria per Cubic Centimeter.		
	Hide Gelatin.	Bone Gelatin.	Ordinary Gelatin.
No. 1	4,170	3,100	3,826
2	2,975	2,960	2,718
3	1,800	1,667	1,488
4	1,800	1,524	1,365
5	1,400	1,246	1,205
6	1,064	1,092	763
7	469	391	219
8	142	148	130
9	120	126	125
10	64	48	42

This introduced a subject which, had it been followed by more extended studies, would have revealed the fact that the gelatin is the most important ingredient of the culture medium, and that different brands of gelatins apparently equal in quality may produce even greater differences in the results than those shown by their experiments.

The comparison of gelatins made by the Committee on Standard Methods of Water Analysis, referred to above, indicated in a general way that the highest results were obtained with those gelatins which had the highest melting-points (see Figure 1). In order to test this more carefully, the writer prepared a batch of nutrient-gelatin in the usual way, divided it into portions, and sterilized these for different lengths of time. This gave a series of media of varying spissitude, but with the chemical composition constant except for such changes as may have occurred during sterilization. These different lots of gelatin were then used to determine the number of bacteria in a sample of Brooklyn tap water, the observations being made in dupli-

cate. The results are shown by Figure 12. It will be seen that the gelatins which had the greatest spissitudes and the highest melting-points gave the highest bacterial counts, and that the counts decreased regularly with the spissitude. The liquefying colonies spread

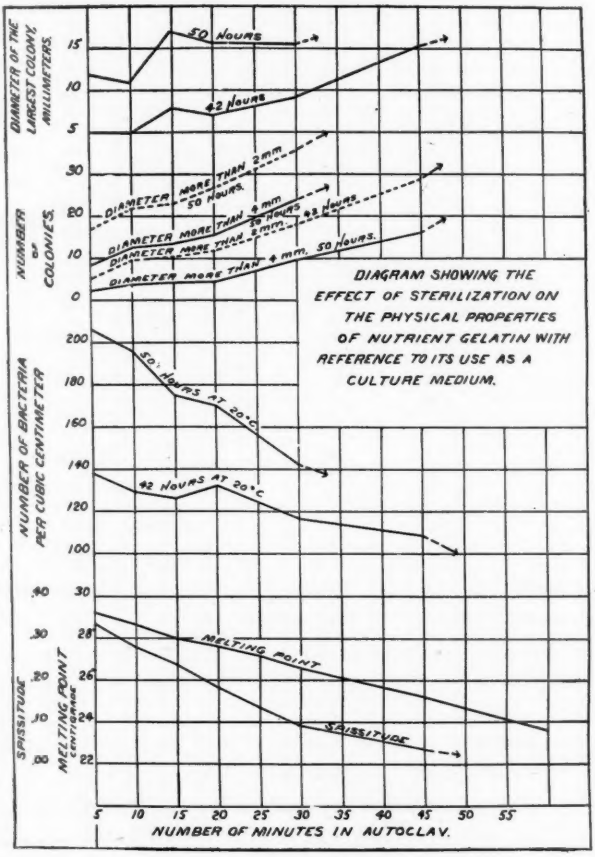
DIAMETER OF THE LARGEST COLONY MILLIMETERS.
50 HOURS
42 HOURS
NUMBER OF COLONIES.
DIAMETER MORE THAN 2 mm. 50 HOURS.
DIAMETER MORE THAN 4 mm. 50 HOURS.
DIAMETER MORE THAN 5 mm. 42 HOURS.
DIAMETER MORE THAN 4 mm. 50 HOURS.
DIAGRAM SHOWING THE EFFECT OF STERILIZATION ON THE PHYSICAL PROPERTIES OF NUTRIENT GELATIN WITH REFERENCE TO ITS USE AS A CULTURE MEDIUM.
NUMBER OF BACTERIA PER CUBIC CENTIMETER
50 HOURS AT 20°C.
42 HOURS AT 20°C.
SPISSITUDE
MELTING POINT CENTIGRADE
MELTING POINT
SPISSITUDE
NUMBER OF MINUTES IN AUTOCLAV.

FIG. 12.—EFFECTS OF STERILIZATION.

more rapidly in the thinner gelatins, as may be seen from the increased size of the colonies shown in the upper portion of the diagram. In the gelatins which had very low melting-points, liquefaction of the entire plate occurred before a count could be made. While it was true that the increase of liquefaction in the case of the

thinner gelatins tended to give lower counts, because of obscuration of the smaller colonies, it was doubtful if this was sufficient to account for the great differences observed; and the slight increase in acidity during sterilization did not explain it. Apparently some change in the character of the gelatin occurred on heating which gave rise to products inimical to the growth of certain of the bacteria present. What these changes were has not been investigated.

Nine lots of nutrient-gelatin were then made according to the standard formula, using the various gelatins given in the preceding list. It was intended that all should have the same reaction, namely + 1.5 per cent.; but partly on account of changes during sterilization, the products, when tested, were found to vary somewhat from that figure. The spissitudes varied with the different gelatins, and it was expected that, as before, the counts would vary accordingly, but such did not prove to be the case. Apparently, the differences were due more to different chemical than to different physical qualities. The highest count was obtained with a gelatin of comparatively low spissitude, namely, No. 6. Neither did the size of the liquefying colonies vary with the spissitude. The results of this comparison are given in Table XIV. All of the tests were made in duplicate.

Other lots of media were then prepared with the same gelatins, but with the beef-infusion omitted, in order that a uniform reaction might be more easily obtained. The reaction was made + 1.0 per cent. Duplicate plates of a sample of Ridgewood water were then made, the results of which are given in Table XV. These showed even greater variations than before, the maximum number after seventy-two hours being fifteen times the minimum; and, strangely enough, these two results were obtained by using different lots of the same brand of gelatin.[1]

[1] It was afterwards learned that Sample No. 3 had been kept in a chemical closet at the bottom of which several bottles of formaldehyde solution were stored. It is possible, therefore, that the gelatin may have absorbed enough of this gas to have a germicidal effect on the bacteria. The absorptive powers of the gelatin molecule are well known, and if the above explanation be the true one, it teaches its own lesson in regard to the storage of gelatin in a laboratory.

TABLE XIV, SHOWING THE NUMBER OF BACTERIA IN BROOKLYN WATER, AND THE SIZE OF THE LIQUEFYING COLONIES WHEN CULTIVATED ON VARIOUS BEEF-PEPTON GELATINS. DATE, FEBRUARY 10, 1902.

Number of gelatin.	Spissitude.	Reaction.	Number of Bacteria per Cubic Centimeter.		Number of Liquefying Colonies Larger than 2 mm. in Diameter.		Number of Liquefying Colonies Larger than 4 mm. in Diameter.		Remarks.
			48 hours.	72 hours.	48 hours.	72 hours.	48 hours.	72 hours.	
1	.21	+1.4	56	83	4	28	1	15	
2	.17	+1.7	40	105	1	18	0	9	
3	.08	+1.4	80	69	13	....	4	28	
4	..	+1.3	93	161	9	26	2	17	
5	.33	+1.5	84	171	14	31	4	21	
6	.13	+1.6	113	210	13	33	3	25	
7	.26	+1.7	66	110	10	29	2	13	Fluorescent colonies very brilliant.
8	.11	+1.4	62	69	12	28	3	22	"
9	.26	+1.7	80	154	10	30	3	15	Fluorescent colonies brilliant.

In this comparison it was noted that gelatins Nos. 2, 5 and 6 contained two colonies of *B. violaceus*; that No. 7 contained one colony, and the others none. There was also a great difference in the occurrence of *Pseudomonas ochracea*, the number of recognizable colonies varying from zero to eleven.

TABLE XV, SHOWING THE NUMBER OF BACTERIA IN A SAMPLE OF RIDGEWOOD RESERVOIR WATER, AND THE SIZE OF THE LIQUEFYING COLONIES WHEN CULTIVATED ON VARIOUS PEPTON-GELATINS. FEBRUARY 18, 1902.

Number of gelatin.	Spissitude.	Reaction.	Number of Bacteria per Cubic Centimeter.		Number of Liquefying Colonies Larger in Diameter than 2 mm.		Number of Liquefying Colonies Larger in Diameter than 4 mm.		Number of colonies of B. violaceus.	Number of colonies of P. ochracea.
			48 hours.	72 hours.	48 hours.	72 hours.	48 hours.	72 hours.		
1		1.0	35	44	6	10	3	5	0	1
2	.10	1.0	105	140	14	20	4	10	2	5
3	.05	1.0	16	17	2	3	0	2	0	0
4	.14	1.0	93	222	15	23	3	7	0	3
5	.10	1.1	87	93	21	24	9	15	2	4
6	.06	1.0	187	258	19	25	7	18	2	11
7	.16	1.1	89	207	10	18	4	9	1	3
8	.08	1.0	75	154	10	16	3	5	0	3
9	.24	1.0	68	156	7	12	1	6	0	0

A sample of ground water was then plated in duplicate with the same set of gelatins. The results are given in Table XVI. In this case no liquefaction occurred in any of the plates, yet the differences in the counts were even greater than before. In some of the gelatins there was but one species prominent, while in others there were two. In some cases the colonies were large and full, while in others they were very small.

Comparison of gelatins Nos. 10 to 15 were then made in the same way with No. 6, which in the previous tests had given the highest figures, and with No. 1. The results are given in Table XVII. In this comparison also No. 6 gave the highest figures. It should be observed that No. 11 gave higher results than No. 1, both of these being made with the same brand of gelatin, but gelatin No. 11 being washed thoroughly and dried before using. It should be noted also that gelatin No. 12 gave a low figure. This was made from a gelatin purified by washing, precipitated in alcohol and afterwards washed and dried.

It is apparent therefore that different gelatins vary greatly in their

nutritive value or in their content of substances prejudicial to the development of bacteria. It is evident also, that bacteria differ in the kind of nourishment required. With certain gelatins some bacteria will thrive while others will not. The nature of the crop depends upon the character of the soil. Just what the conditions are that determine the value of a gelatin for quantitative bacteriological work are still obscure, and are now the subject of investigation. From what has been done, however, it is probable that no one kind of gelatin will support the growth of *all* of the common bacteria found in water to such an extent that their colonies are visible after seventy-two hours at 20° C., and therefore that no gelatin can give the true number of bacteria in water. All results by this method must be considered as having but a relative value, though under the most favorable conditions the counts probably approach the truth with an accuracy sufficient for practical purposes. It is evident also that the results obtained by using different kinds of gelatin are not fairly comparable, —and it seems probable that one cannot even depend upon different lots of gelatin made by the same manufacturer. In order to secure results which shall be fairly comparable it would appear to be necessary to use the gelatin not only of *one manufacturer* but also of *one particular mixture.* It is possible that some manufacturer may be found who will undertake to prepare a special mixture of gelatin of proper quality especially for the bacteriological profession and to make the mixture in such a quantity that it will serve all the bacteriologists engaged in water analysis for several years. Then at the end of that time, if sufficient notice is given, this gelatin may be duplicated with considerable closeness. In the present state of our ignorance of the subject, this appears to be the best that can be done to secure uniform results.

TABLE XVI, SHOWING THE NUMBER OF BACTERIA IN A SAMPLE OF GROUND WATER WHEN CULTIVATED ON VARIOUS PEPTON-GELATINS, FOR NINETY-SIX HOURS at 20° C., FEBRUARY 18, 1902.

Number of Gelatin.	Spissitude.	Reaction.	Number of Bacteria per cubic centimeter.	Remarks.
1	—	1.0	220	One species chiefly. Good growth.
2	.10	1.0	190	One species chiefly. Poor growth.
3	.05	1.0	5	Poor growth.
4	.14	1.0	170	One species chiefly. Good growth.
5	.10	1.1	330	Two species prominent. Good growth.
6	.06	1.0	430	Two species prominent. Good growth.
7	.16	1.1	170	One species prominent. Poor growth.
8	.08	1.0	290	Two species present. Poor growth.
9	.24	1.0	20	Poor growth.

TABLE XVII, SHOWING THE NUMBER OF BACTERIA PER CUBIC CENTIMETER IN VARIOUS SAMPLES OF WATER WHEN CULTIVATED ON VARIOUS PEPTON-GELATIN SOLUTIONS FOR FORTY-EIGHT HOURS AT 20° C.

Number of Gelatin.	Spissitude.	Reaction.	NUMBER OF BACTERIA PER C.C.		
			Sample No. 1.	Sample No. 2.	Sample No. 3.
1	.12	+1.0	310	420	370
6	.09	1.0	800	600	480
10	.18	1.0	675	420	300
11	.14	1.0	675	550	500
12	—	1.0	390	—	—
13	.16	1.0	775	410	360
14	.17	1.0	675	440	380
15	.12	1.0	550	390	380

Effect of the Character of Gelatin on its Use as a Differential Test in the Study of Bacteria.

From the earliest days of bacteriology the "stab culture" in gelatin has been regarded as an important differential test in the study of bacteria, and the character of the growth along the stab has been considered as constant for each particular species. To a certain extent this is true, but in some cases the character of the medium exercises an important influence on the nature of the growth. This was strikingly

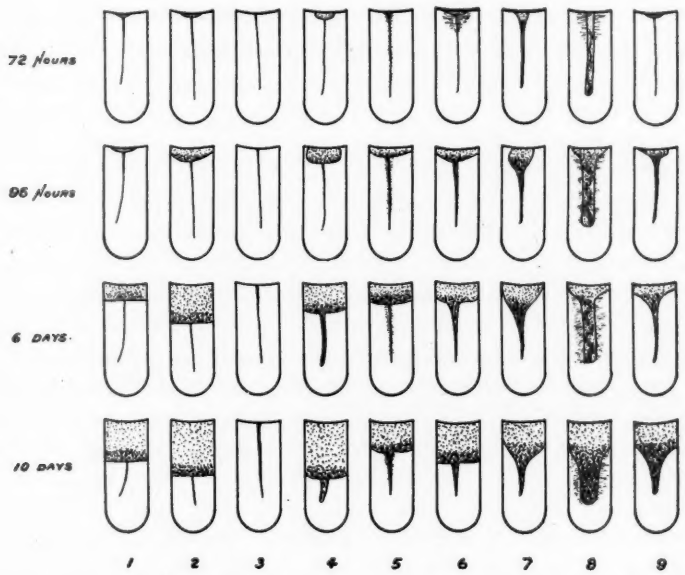

Fig. 13. — Stab-Cultures of Bacillus Liquefaciens in Various Pepton-Gelatins.

exhibited in the following comparison of the growth of certain bacteria in the nine lots of pepton-gelatin already alluded to.

Figure 13 shows the growth of *Bacillus liquefaciens* in these nine gelatins after different periods of time. It will be seen that the growths differed both in their character and in their rate of progress. For example, in No. 3 the growth was very slight; in Nos. 1, 2, 4, 5, and 6 it was stratiform; in Nos. 7 and 9 it was infundibuliform; while in No. 8 it was saccate. Furthermore, in Nos. 5 and 8 the growth was somewhat villous. These characteristics, therefore, cannot be

depended upon as true marks of differentiation in the case of this species.

In the case of *Bacillus fluorescens-liquefaciens* the shape of the stab-growth was about the same in all cases, but the extent of the fluorescence differed materially. Thus in Nos. 1 and 3 it was well marked; in No. 1 it was greenish, while in No. 3 it had a decided orange cast; in Nos. 2, 4 and 8 it was but slightly noticeable, while in Nos. 5, 6 and 7 it was entirely absent, even after ten days. (See Figure 14.)

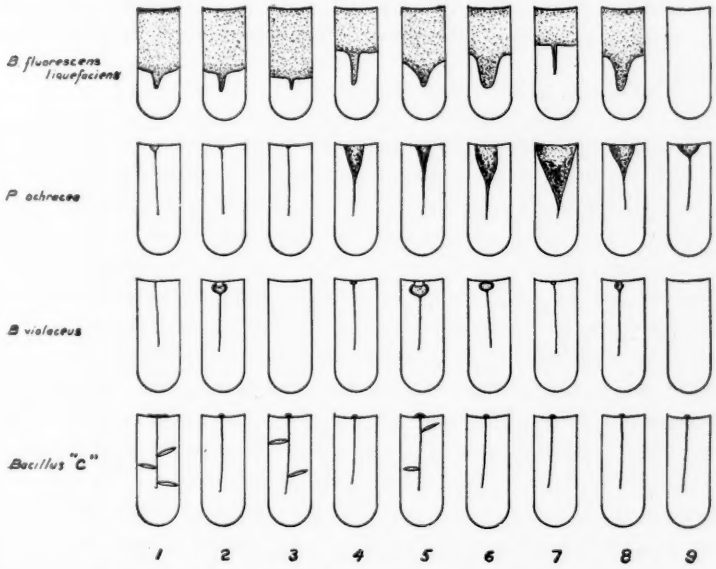

FIG. 14. — STAB-CULTURES IN VARIOUS PEPTON-GELATINS.

Pseudomonas ochracea gave growths which ultimately were much alike in their general appearance, but which differed materially in their rapidity of development. A violet form (probably *Bacillus violaceus*) also showed great differences in its rapidity of growth in the different gelatins. It developed most rapidly in Nos. 2, 5, 6 and 7; in No. 8 the growth was very slight; in No. 1 there was no growth. It will be remembered that on the gelatin plate violet colonies were observed as gelatin Nos. 2, 5, 6 and 7, but not on the others. (See page 154.)

A non-liquefying species recorded as Bacillus "C" gave stab-growths which agreed in their general character, but which differed in that Nos. 1, 3 and 5 showed cracks due to gas-production, while in the others no gas was produced (Figure 14). Inasmuch as no beef-infusion was used in these medias, but only pepton, the fermenting substance must have been present in the gelatin itself.

It is evident therefore that in qualitative as well as in quantitative bacteriological work the character of the gelatin is most important.

The spissitude of the gelatin seemed to have but little effect in determining the nature of the growth along the stab, but in the case of the liquefying bacteria the growth was most rapid in the softer gelatins.

Resumé.

Experimental studies have been presented in this paper to show that the character of the gelatin used in culture media has a most important influence upon qualitative and quantitative bacteriological work, and that for different observers to obtain results which may be fairly comparable it will be necessary to use culture media made from one and the same lot of gelatin. It will be necessary, also, to follow a most rigid system in the preparation, sterilization and use of nutrient gelatin, in order that its physical condition may be the same in all cases. The chemical characteristics of different gelatins with reference to their use in culture media are not discussed, as investigations in that direction are not completed, but the effect of the physical condition of the culture medium on bacterial growth is pointed out. Incidentally the viscosity, melting-point and spissitude of gelatin solutions have been described in some detail, and a new form of spissimeter and a new method of stating the results of spissitude measurements have been suggested.

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ORIGIN AND RELATIONS OF THE AURIFEROUS VEINS OF ALGOMA (WESTERN ONTARIO).

By W. O. CROSBY.

Introduction.

Observations made some years ago in Eastern Algoma, in the vicinity of Sudbury, Whitefish, and elsewhere, and more recently in the new Michipicoten District of Central Algoma, supplemented by a study of the descriptions of the auriferous deposits of Algoma and Nipissing, and notably of the Rainy Lake and Lake of the Woods districts, by Dr. A. P. Coleman,[1] have revealed for this vast area, as also noted by Dr. Coleman, a surprising uniformity in the character of the gold-bearing deposits, as regards alike their geological relations and their structural and mineralogical features. The fact has also been brought out with great distinctness that these deposits, which belong in a general view to the class of veins, are, in the main at least, exogenous rather than endogenous in origin; and the correlation of structure and origin is the main purpose or inspiration of this paper. It is proposed to present: first, the writer's observations in the Michipicoten District; second, a general summary of Dr. Coleman's observations and conclusions, so far as they bear upon the genesis of the veins; and, third, a comparison, in the light of all the pertinent data, of the endogenous and exogenous theories of vein formation, as applied to the auriferous veins of Algoma.

Auriferous Veins of the Michipicoten District.

My observations in this field were limited to the vicinity of Lake Wawa, the south end of which is about six miles in a direct line from Michipicoten harbor; and extended to distances of three to four

[1] Annual reports of the Ontario Bureau of Mines.

miles southeast and south of the lake. Like most of Algoma, this section, although topographically rugged, is heavily forested; the depressions are largely of a swampy character, and a thick bed of moss covers both hills and valleys, so that prospecting is difficult and opportunities for geological investigation are largely confined to the lake shores and the mines. The Michipicoten Mining District embraces the Huronian area which, as recently mapped by Coleman, extends from the northeastern corner of Lake Superior northeastward to the Canadian Pacific Railroad. As admirably exposed in the vicinity of the Helen Iron Mine and elsewhere, the Huronian series consists of a great and complex development of slates, quartzites, schists, etc., besides the great beds of pyrite and siderite in which the iron ores have their origin. Intersecting these undoubted sedimentary deposits — the true Huronian — is an extensive complex of igneous rocks, of both acid and basic types, but consisting chiefly of granites (often gneissoid), felsites (often altered to sericite schists), diorite and gabbro, which are also extensively altered to massive or schistose greenstone. It is perhaps improbable that the igneous rocks all belong to the same or nearly the same period; but there seems to be no reason to doubt that the diorites, of which the gabbros are, perhaps, a more basic phase, are, as usual, closely related to the granites, representing a differentiation and earlier crystallization of the same magma. The diorites grade into the granites and have been, along their common boundaries, extensively penetrated and enclosed by the latter. In the past the diorites have usually been referred to the Huronian, and the granites to the Laurentian; which, as noted later by Lawson, Adams, Coleman and others, makes the Laurentian clearly newer than the entire Huronian series. The gneissoid or foliated structure of the granites, as of the felsites and other types, may, in part, be regarded as secondary and referred to dynamic metamorphism; but in large part also, it is clearly original and essentially flow structure developed under pressure. The latter explanation would account especially for the very prevalent gneissoid borders of the granite areas.

In the vicinity of Lake Wawa, the granite is prevailingly a rather fine-grained, biotitic variety, passing at some points into a granite porphyry, the prominent rounded blebs of quartz suggesting a typical quartz porphyry. This variety is well developed near the southeastern shore of the lake. In the diorite also, the dark constituent

is chiefly biotite, and, in part, as in Mackey's Point on the southeastern shore of Lake Wawa, quartz is a prominent constituent, giving a more or less typical quartz diorite.

The veins of the Wawa area clearly belong to two distinct types, which may be briefly described as (1) branching gash veins in the massive diorite, and more rarely in the massive interiors of the granite areas; and (2) non-branching, but usually sheeted or composite, veins occurring in the foliated zones of the granite, and very commonly on the border of the diorite. It can hardly be doubted that, in this instance at least, the basic rocks, referred to here collectively as diorite, have been a much more important factor than the granite in

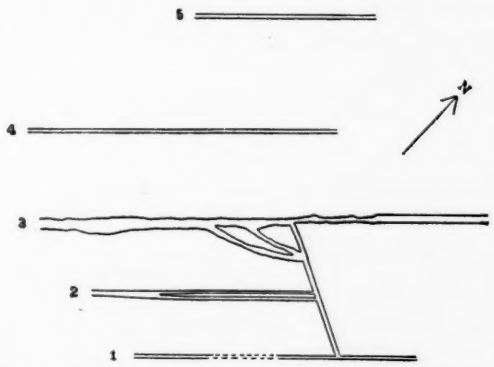

FIG. 1.—GROUP OF VEINS ON THE SOUTH SIDE OF MACKEY'S POINT, LAKE WAWA.
Scale: 1 inch = 60 feet.

the formation of veins; although the foliation of the latter rock has determined the locus of one important structural type.

Gash Veins. — Probably no clearer or more instructive examples of the veins of this type are exposed in this area than those on Mackey's Point on the southeast side of Lake Wawa, where the first discovery of gold in the Michipicoten District was made. Mackey's Point, which is embraced in mining lot Y 103, is a sharp ridge of massive, black, biotitic, quartz diorite, jutting into the lake. On the southern slope of the headland is the branching series of veins shown in Figure 1. This is seen to consist chiefly of five parallel veins, from 1 to 4 feet in thickness and having a general northeast-southwest trend, and a northwest dip, which varies from 35° to 70°. But veins 1 and 3 are connected by the cross vein which

does not intersect either, and from which vein No. 2 is best regarded as a double branch, since no trace of it is to be seen on the northeast side of the cross vein. The abrupt variations in width of vein No. 3 and its branching connection with the cross vein are significant. Figure 2 shows in detail the branching termination of vein No. 2. In both form and grouping these are typical gash veins, understanding by that term, veins formed in joint cracks without important displacement or faulting of the walls. Owing to the extremely massive character of the diorite and the absence of displacement, enclosed fragments of the wall rock are few, except where, as shown in the figures, blocks of the country rock, often of considerable size, are isolated by the branching of the veins. That the fissures

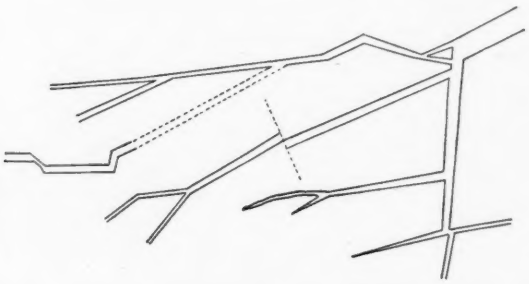

FIG. 2.— DETAILED BRANCHING OF ONE OF THE VEINS SHOWN IN FIGURE 1.
Scale: 1 inch = 10 feet.

are not due to faulting movements is shown by the general absence of slickenside and selvage phenomena, the vein material being, as a rule, firmly welded to the walls. In general, near all the larger veins, and there only, as a rule, the diorite is more or less distinctly chloritized, this alteration yielding at some points a typical greenstone, and being particularly noticeable in the enclosed masses of diorite. The walls of the veins are, in most cases, very regular and sharply defined, and the contacts are not suggestive of a metasomatic replacement of the diorite. The veins consist chiefly of massive quartz in which the sulphides — pyrite and pyrrhotite — occur sparingly and very irregularly, considerable bodies of quartz being, apparently, entirely free from the sulphides. Alteration of the vein contents since the close of the Ice Age has been slight; and the unoxidized sulphides are usually found within a few inches or a

foot or two of the surface. Tourmaline, in slender, black needles imbedded in the solid quartz, is a common, though rather inconspicuous, feature of the veins; and green chlorite, evidently derived from the dark constituents of the diorite, occurs in the significant forms shown in Figure 3. A normal banded or comb structure and vugs or pockets are wholly wanting; and the only thing to be noted in the way of a definite internal structure is the dark line, sometimes to be observed, as in Figure 3, dividing the vein midway

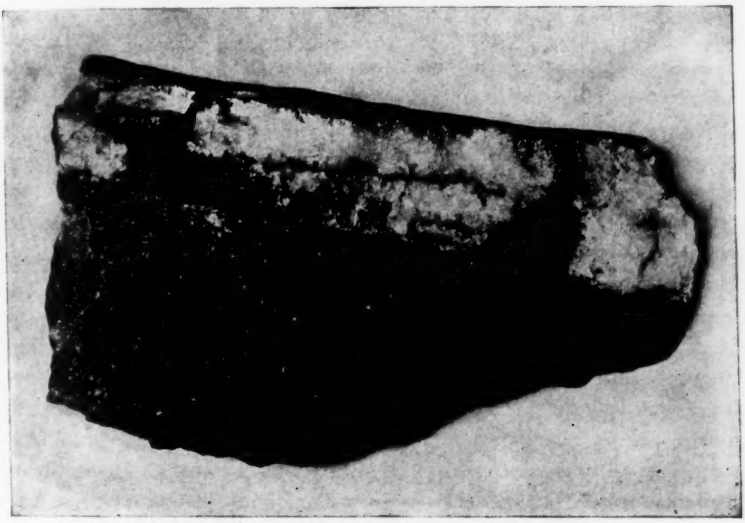

FIG. 3. — QUARTZ VEIN ONE INCH WIDE IN THE DIORITE OF MACKEY'S POINT.[1]
The median line represents the primitive crack along which the formation of the vein began; and masses of chlorite are seen extending outward from this line to the walls.

and parallel with its walls. Although, as stated, there is no true selvage, the diorite, where most completely altered to greenstone, may be divided, as usual, by slickenside surfaces, which may, or may not, be parallel with the walls of the vein.

The best explanation of these veins seems to be that they are not only true gash veins formed in the joint cracks of the diorite, but that they are also a product of segregation from the diorite; or, in other words, they have been formed by lateral secretion. We have

[1] The photographs for this and all of the following illustrations were kindly furnished by Mr. John L. Gardner.

an abundant source of the free silica or quartz in the greenstone alteration of the diorite; and the sulphides, both pyrite and pyrrhotite, are throughout a notable feature of the diorite. The question next arises as to whether they are in origin endogenous or exogenous. That is, were they formed in preëxisting, open fissures; or like a concretion or a root in the soil, did they make room for themselves as they grew? Quite certainly, many of the smaller veins of this class are completely isolated in the diorite, occurring as thin, lenticular sheets of limited extent in depth and strike, and lateral secretion is, obviously, the only permissible explanation for these.

An exogenous origin or growth from the middle outwards is indicated by the massive structure, the absence of a true comb structure and vugs, and the angular inclusions of the country rock unsupported by the wall. The general idea is that the veins began to form in incipient cracks, or possibly along merely potential fractures, the formation of which may be supposed to imply relief of pressure which would favor the deposition of dissolved minerals. The veins, thus inaugurated, are widened and extended as long as the supply of material from the enclosing country rock is kept up. In the case of the examples represented by Figure 3, the dark median line represents the primitive crack from which the vein has grown either way, forcing the walls apart; and the enclosed masses of chlorite do not represent inclusions of the diorite, but rather a free crystallization of chlorite derived from the wall-rock, localized in obedience to the principle of segregation, and showing a tendency to increase in breadth outwards, as if the supply of chlorite became more abundant through a gradual acceleration of the greenstone alteration of the diorite. It is noticeable that this pure, soft, green, crystalline chlorite is strictly confined to the veins, and separated by a sharp line of demarcation from the partially chloritized diorite of the wall. The sulphides, although distributed very irregularly, as regards the major dimensions of the veins, are found mainly near the walls, suggesting very plainly that they should be, like the chlorite, regarded as a recrystallized residuary portion of the diorite. Again, the sulphides are clearly most abundant in that part of the diorite immediately bordering the vein, and farthest advanced in chloritization; and there is thus some ground for the view that they are in part derived from the ferruginous silicates, as has often been suggested for other and more important occurrences.

Accompanying the quartz veins, and yet essentially distinct from them, are veinlets of a crystalline, ferruginous carbonate, which proves on examination to be ankerite. Unlike the quartz veins and veinlets, these often have decidedly irregular outlines; and the contacts with the diorite, although sharp, as shown in Figures 4 and 5, suggest replacement, rather than a mechanical displacement or separation of

FIG. 4. — BRANCHING VEINLETS OF ANKERITE IN THE DIORITE OF MACKEY'S POINT. Three-fourths natural size.

the walls. The discontinuance of the ankerite vein shown in Figure 5, without any signs of a break or slip in the diorite, affords a specially clear indication of replacement. The ankerite veins or vein-like substitutions are also a feature of the massive granite, having in some cases a thickness of from one to several feet, and showing a perfect gradation from thin impregnations to complete replacements.

That these carbonate replacements have been derived from the immediately surrounding wall-rock there can be no question. At the surface they are usually oxidized to yellow ochre. The carbonate veinlets find a ready explanation in the percolation of meteoric waters carrying carbon dioxide and free oxygen and carbonating the lime, magnesia and ferrous iron of the silicates, and sulphating

FIG. 5. — VEIN OF QUARTZ AND BLIND VEINLET OF ANKERITE IN DIORITE OF MACKEY'S POINT. Two-thirds natural size.

the disseminated pyrite, the ferrous sulphate reacting with the carbonates of the alkaline earths to increase the amount of ferrous carbonate or ankerite. The silica set free by the carbonation of the protoxide bases of the silicates probably contributes to the formation of the quartz veins.

In the massive interiors of the granite areas the gash veins occur less frequently than in the diorite, but vary in width up to five and even ten feet. Also they are less given to branching and partake more of the character of true fissure veins. Mineralogically and structurally, however, there is no essential difference; and in the massive character of the quartz and absence of true banding, combs or vugs, as well as in the isolated inclusions of the wall-rock,

they bear evidence of their exogenous origin. The gash veins are clearly not to be sharply distinguished in character or origin from the sheeted veins ; and, doubtless, many intermediate forms occur.

Sheeted Veins. — As previously stated, the veins of this type are confined to the foliated zones of the granite. These zones, whether they are to be interpreted as original flow structure or the product

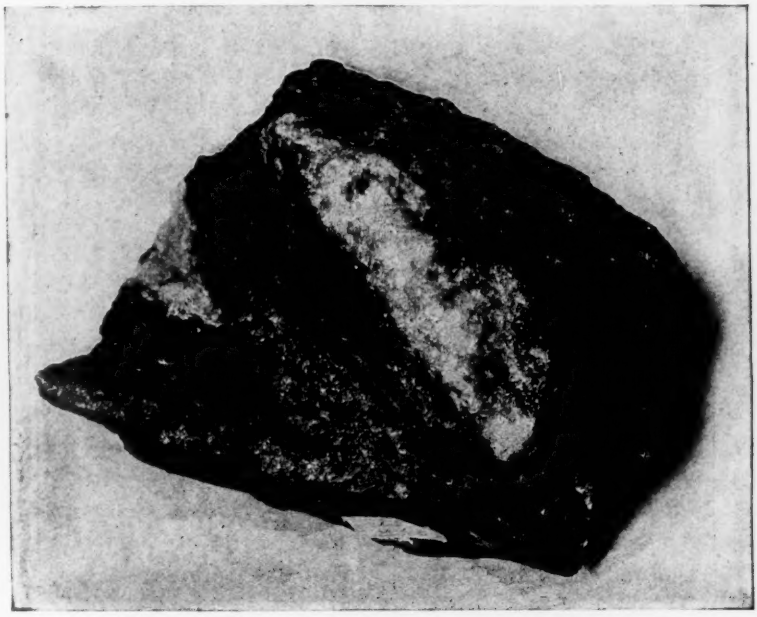

FIG. 6. — LENTICULAR VEINLETS OF QUARTZ IN FOLIATED GRANITE FROM THE HORNBLENDE MINE. Two-thirds natural size.

of subsequent shearing, are in either case essentially the product of dynamic metamorphism and appear to be chiefly confined to the vicinity of the granite-diorite contacts, or at least to the contacts of two different products of the general granitic magma. The veins agree closely with the foliation of the granite in both trend and dip and are rarely vertical; and, as is customary with veins in foliated or schistose rocks, they exhibit in general outline and in detail a tendency toward a lenticular form, as is very strikingly shown on a small scale in Figures 6 and 7. These are relatively short, thick and

distinctly overlapping lenses; and their wedge-like relations to the foliated structure of the granite is very noticeable. In other instances, and in most of the larger veins, the enclosed plates and slabs of the wall-rock are a very important feature. These are always parallel with each other and with the wall of the vein, which may consist in large part of these inclusions or partings, actual veins, and sometimes the same vein, showing every gradation from a solid vein

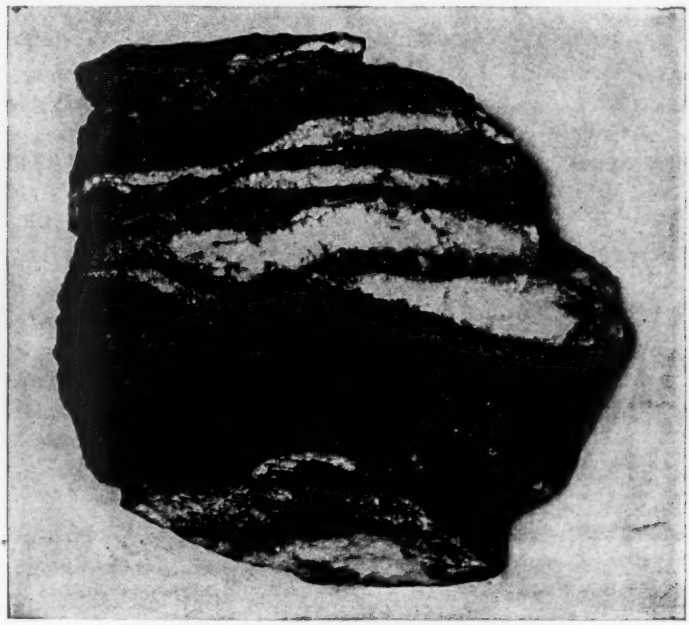

FIG. 7. — LENTICULAR VEINLETS OF QUARTZ IN SCHISTOSE AND CHLORITIC GRANITE FROM THE MINTO MINE. Two-thirds natural size.

of quartz to a band of schist with interlaminated veinlets, as represented on a small scale in Figure 7. These inclusions are very commonly isolated by the vein matter and afford one of the most cogent arguments for the exogenous origin of these veins. The veins vary in width up to ten feet or more, although comparatively few are above five feet; and the proportion of vein matter to inclusions varies inversely as the schistosity of the wall-rock.

As regards the character of the vein matter, the sheeted veins

have much in common with the gash veins. Quartz is the chief gangue mineral and the sulphides include pyrrhotite and pyrite chiefly, with some chalcopyrite and occasionally small amounts of sphalerite and galenite. The most noticeable difference in the composition of the two classes of veins is found in the fact that the crystalline carbonate or ankerite which is wanting in the gash veins, but forms associated substitution veinlets, is in many cases a direct constituent of the sheeted veins, in which it sometimes occurs in great abundance. Its relations to the quartz, as shown in Figure 8, prove

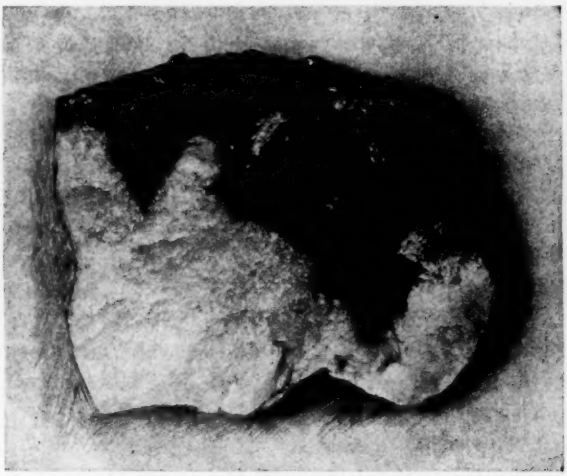

FIG. 8. — ASSOCIATION OF QUARTZ AND ANKERITE IN THE MINTO MINE.
All the dark material is coarsely crystalline ankerite superficially brown by oxidation. Three-fourths natural size.

their contemporaneous deposition. It is very coarsely crystalline or sparry, while the quartz, as before, is entirely massive; and, as in the true gash veins, true banding, comb structure and vugs are conspicuous by their absence. Tourmaline, in clusters of slender, black needles shooting through the quartz, occurs more or less sparingly in both classes of veins and has given the name Hornblende Mine to one of the mines in the vicinity of Lake Wawa. In both types of veins, also, the gold is chiefly free, in the main fairly coarse, and very irregularly distributed. To determine the percentage of free gold, twelve samples ranging in value from a trace of gold to 2.60

ounces per ton, and averaging .90 ounce per ton, were treated by amalgamation; and the gold saved was found to range from 23 to 100 per cent. of the assay value, the average saving being 77 per cent., and distinctly higher for the high than the low values.

Summary of the Structural Features of the Auriferous Veins of Algoma. Compiled from Reports by A. P. Coleman.

It is proposed to cite here only such portions of Dr. Coleman's reports as appear to be specially pertinent in their bearing upon the structure and origin of the veins. In the interests of brevity the citations often are not literal or continuous, and hence quotation marks are omitted.

General Observations and Conclusions. — All the important gold districts of Algoma, or Western Ontario, are in the Huronian series, if we assume that Lawson's Keewatin is in reality of that age.[1] Gold has been found in a stretch of country reaching from near the Manitoba boundary for a distance of two hundred and sixty miles; and over a breadth of one hundred and twenty or thirty miles. Gold in paying quantities will not, however, be found everywhere within this area; for probably two-thirds of this territory is Laurentian, which has never proved to be auriferous except near the margin of the Huronian.

In general, the granite forming the country rock for gold-bearing fissure veins is apt to be modified by shearing and weathering into a greenish rock, often called protogine, in which the mica or hornblende of the original granite is changed into sericite or chlorite. Generally the change has gone still farther at the immediate edge of the vein, the feldspar being changed to sericite and other products, the rock taking on the look of a schist, probably as the result of faulting when the fissure was formed and of the action of circulating water during the filling of the vein.[2]

It was formerly held that gold and other metals were to be looked for in Ontario only in the Huronian, the Laurentian being considered barren. It has been found, however, that some of the most promis-

[1] Fourth Report Bureau of Mines, Ontario, 1894, p. 39.

[2] Fifth Report Bureau of Mines, Ontario, 1895, pp. 86–87.

ing gold deposits occur either in granite or in gneiss; and further, that both in the granitoid rocks and the green Huronian schists the best veins or other ore deposits occur at or near the contact of the two kinds of rock.

The source of the gold deposits in our rocks is not easy to determine. The fact that they almost all occur near the contact of granitoid eruptive rocks with the Huronian suggests that one or other of these rocks furnished the original supply of gold, probably by lateral secretion. In the case of small lenses we cannot assume fissures reaching to great depths, so the adjoining rocks must have contained the gold afterwards concentrated in the fissures opened by the pushing up of granite masses into the Huronian schists. Gold has frequently been found in the country rock immediately beside veins, but probably this has been deposited from the solutions circulating in the veins themselves.

The auriferous fahlbands of the Huronian may have been supplied with the metal in the beginning, or they may represent shear zones where the rocks have been so shattered as to allow solutions to percolate and deposit sulphides with gold. The eruptive masses carrying gold almost certainly brought the metal up with them from the depths where they originated.

The great quartz lenses of the Sultana Mine are found at the northwestern edge of an oval boss mapped by Lawson as Laurentian. The enclosing rock, mapped as Keewatin, is green, fine-grained and not distinctly schistose. The boss of Laurentian consists in its central part of coarse-grained, gray granite, with phenocrysts of orthoclase half an inch wide. Going westward towards the edge of the boss, the granite slowly becomes schistose, showing distinct parallelism of structure, and the porphyritic feldspars are rolled out into imperfect *augen* or lens-shaped portions. Near the Crown Reef the coarse gray gneiss passes rapidly into the fine-grained, dark gray gneiss of a very schistose kind, and then into very fine-grained brown-black mica schist with crumpled strips or lenses of quartz. The large quartz lenses in which the mine is worked lie in this schistose edge of the boss, and contain many thin sheets or thicker bands of the dark schist.

As one approaches the edge of the boss, the signs of crushing and shearing grow intense. The quartz shows "mortar" structure, being crushed into fine particles enclosing larger fragments. The

feldspars are broken and portions shifted asunder, calcite sometimes serving as cement. At the very edge, the quartz is rolled out into streaks of crushed particles, the feldspar largely changed into a mixture of sericite and brown mica, and calcite and sulphides appear in considerable quantities. At the extreme edge little is to be seen in thin sections but brown biotite in minute scales.

It may be imagined that the granite, while still somewhat plastic, *i. e.*, not yet cooled to the point of consolidation, pushed up through the Huronian rocks, more or less shattering them and dragging out its own margin into the present schistose form. Before the process was complete the sheared edge of the mass was solid enough to allow the formation of great cavities between the layers of gneiss and mica schist. Here the fluids of the now solid but still hot granite could circulate, depositing the auriferous quartz.[1]

True Fissure Veins. — True fissure veins are most commonly found in the areas of eruptive granite, and form important gold deposits in many cases, having a continuity and uniformity not usually found in the other types. The best known examples are those of Shoal Lake on Seine River, where an area of coarse granite six miles in length by one or two in breadth contains scores and perhaps hundreds of well-defined veins running in various directions. The granite is of the green "protogine" variety, a very siliceous plagioclase, or soda granite, in which there is much sericite; and it pushes up through Huronian rocks that had been greatly disturbed in ages long previous to the eruption.

Many of the fissure veins in the Huronian schists are narrow and very crooked and irregular, and they are often associated with larger bedded veins. In that case the narrow cross (fissure) veins are generally much richer than the others.[2]

Bedded or Lenticular Veins. — The bedded, lenticular or segregated veins belong especially to schistose rocks, just as the true fissure veins are confined chiefly to massive rocks. They are generally lenticular in shape, and when a lens runs out another often replaces it a little to one side. Frequently several layers of schist are more or less parted, bands of quartz of varying thickness filling the spaces between, when the ore body may have quite ill-defined limits;

[1] Sixth Report Bureau of Mines, Ontario, 1896, pp. 119, 120, 121, 122, 123.

[2] *Ibid.*, pp. 115–116.

but in many cases the lenses have tolerably definite walls, though seldom so distinct as in the case of true fissure veins, and the two kinds are not to be looked on as totally different in character, since both must have been at one time open fissures through which water circulated. But they are not usually so continuous horizontally and vertically as true fissure veins and the quartz is more often quartzitic in look than in the other class.

The term "segregated," frequently used for these ore deposits, seems less correct than "bedded," since it implies a mode of formation by lateral secretion or segregation which is by no means proved. In fact, it is probable, as stated above, that these veins are filled with mineral matter very much as fissure veins are.[1]

A most interesting example of lenticular veins is found in the Sultana Mine, the country rock being a gray schist probably formed by the shearing of a mass of porphyritic gneiss as it pushed up through Huronian rocks.

Veins of this description in Huronian rocks are by all means the commonest form of gold deposit in Ontario. They vary greatly in size, shape, and importance. Often one finds a great number of small crumpled stringers of quartz with schist between, the whole uniting at some point perhaps to larger quartz masses, but without any well-marked walls. In other cases there are well defined bands of quartz with fairly good walls, and traceable even for miles.[2]

Fahlbands. — Fahlbands, or bands of schist impregnated with pyrite and other sulphides, are not infrequent in the Huronian rocks of Ontario. Frequently some quartz is embedded between the pyritous schists, forming a connecting link with certain bedded veins.[3]

Rainy Lake Region. — The more important ore deposits in the neighborhood of Rainy Lake are of two kinds, bedded or segregation veins and true fissure veins, the latter being much less common than the former.[4]

At the little American Mine, Keewatin schist encloses numerous small bedded veins of quartz, with iron and copper pyrites and a little white calcite.[5]

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[1] Fourth Report Bureau of Mines, Ontario, 1894, p. 50.
[2] Sixth Report Bureau of Mines, Ontario, 1896, pp. 116–117.
[3] *Ibid.*, p. 118.
[4] Fourth Report Bureau of Mines, Ontario, 1894, p. 50.
[5] *Ibid.*, p. 53.

The Lyle Mine on Dryweed Island is operating on bedded quartz veins in green Keewatin schist. The quartz contains pyrite, a carbonate, probably dolomite, and some black tourmaline.[1]

Between Bad Vermilion and Shoal Lakes the granite, which is coarse grained and a typical granite, flesh colored and with black mica in unaltered parts, becomes modified into a greenish, chloritic granite, the so-called protogine, near the vein ; and at its very edge is changed into a greenish, schistose rock, consisting of quartz veins and chlorite or sericite, but almost entirely free from feldspar. This band of modified granite probably results from a shearing motion which had ground down the softer minerals when the fissure, no doubt accompanied by faulting, was formed. The circulation of hot water in the fissure probably completed the change of the crushed feldspar into sericite masses.[2]

Lake of the Woods Region. — Almost all the gold locations in the Lake of the Woods region appear to lie along the disturbed margin of the Laurentian and Keewatin ; gold-bearing veins, both bedded and true fissures, occurring within the Laurentian gneisses, as well as the Keewatin schists, as though they partook more or less of the nature of contact deposits. If the gneiss is really a species of plutonic rock which has heaved up, fissured, and folded the already solidified Keewatin, the occurrence of veins along and near the contact seems very natural, for these convulsions must have opened up many channels for the flow of water, highly heated by the molten or half-molten rock beneath, the very conditions favorable to the deposition of ores. In this respect the Lake of the Woods gold region seems to differ greatly from that of Rainy Lake, where scarcely any important gold-bearing veins have been found near the edge of the Laurentian, and none whatever within the Laurentian.[3]

At the Keewatin Mine, Big Stone Bay, small bedded and gash veins occur in green schist. The quartz contains a little black, slaty rock, pyrite, and slender, radiating prisms of tourmaline.[4]

The deposit of the Scramble Mine is an excellent example of a fahlband, though some quartz is embedded between the laminæ of

[1] Fifth Report Bureau of Mines, Ontario, 1895, p. 63.
[2] Fifth Report Bureau of Mines, Ontario, 1895, p. 66.
[3] Fourth Report Bureau of Mines, Ontario, 1894, pp. 68–69.
[4] Sixth Report Bureau of Mines, Ontario, 1896, p. 97.

schist. The rock is mica schist or mica chlorite schist, and the vein rock differs from the adjoining country in being heavily impregnated with pyrites.[1]

Upper Seine Region. — On Reserve Island a bedded vein contains white and greenish quartz, with some galena and iron pyrites. The vein has well-slickensided walls, at least in some parts, and the country rock is protogine granite, but in places chlorite schist shows itself associated with the quartz. Another vein of white quartz contains some green schist and has one good wall of granite.[2]

At Sawbill Lake the vein is about four feet wide. The quartz contains pyrite, chalcopyrite, galena and some free gold. The country rock is an imperfectly foliated gneiss. A microscopic examination shows that much of the rock is a tolerably fresh, biotite granite gneiss, but that the parts near the vein are greatly altered.[3]

Wahnapitae Region. — In general there are considerable differences between the geology of this gold region and that around Lake of the Woods and Rainy Lake. In the more westerly gold district quartzites are rare, and graywacke only moderately prevalent, while green schists are the commonest rocks. As a general rule, too, in the western region gold is found chiefly near the contact of Huronian with the Laurentian gneiss and granite. Here, however, gold deposits are found eight or ten miles away from the nearest Laurentian contact.[4]

At the Mammoth Vein, the white quartz, sometimes fifty or more feet in width, can be followed more than a quarter of a mile. With the quartz is much of a coarsely crystalline carbonate, weathering brown, and a few sulphides. In large vugs, immense crystals of quartz with much-corroded surfaces are found.[5]

Discussion.

The foregoing citations from Dr. Coleman's reports suffice to show that the vein phenomena of the Michipicoten district are not local or unique, but on the contrary, are fairly typical of the whole region, or at least of the entire length and breadth of Algoma. The uni-

[1] *Ibid.*, p. 108.
[2] Sixth Report Bureau of Mines, Ontario, 1896, pp. 74–75.
[3] *Ibid.*, p. 76.
[4] Report Bureau of Mines, Ontario, VII, Part II, 1898, p. 136.
[5] *Ibid.*, p. 138.

formity of the general geologic conditions throughout this vast area is matched by the uniformity of the auriferous veins, and, perhaps in less degree, of other classes of ore deposits. Everywhere are recognized two main types of veins, Dr. Coleman's fissure veins clearly corresponding to what I have described as gash or joint veins.[1] And the gash veins and sheeted or bedded veins are alike and always characterized by massive quartz, free from true comb structure and vugs, accompanied in many cases by sparry, ferruginous carbonates and ferruginous sulphides, with occasional slight developments of other sulphides and of slender crystallizations of tourmaline. Also, the gold occurring in the veins of either type is chiefly free, often fairly coarse, and likely not to be evenly distributed, limited bodies of rich quartz often alternating with much larger bodies of very low grade or nearly barren quartz. In general, the enclosing formations, at least in the immediate proximity of the veins, exhibit more or less metamorphism — dynamic (shearing) or chemical (chloritization), or both. The dynamic metamorphism has to a large extent determined the locus of vein formation (sheeted or bedded veins) and the chemical metamorphism has supplied in ample measure the free silica, carbonates and sulphides, and probably the gold and other minerals, required for the formation of the veins.

The origin of both types of veins by segregation, or lateral secretion from the enclosing formations, is indicated alike by the structure and the composition ; and ascending solutions have apparently played no important part here. In fact, they are inconceivable for the isolated lenses and fahlbands, as noted by Coleman, and are demanded by no known facts, lateral secretion fulfilling perfectly, so far as we know, all the requirements of a complete explanation. Elsewhere, however, as we have seen, Coleman questions the sufficiency of the lateral secretion theory, even for the sheeted veins, and suggests that they are probably filled with mineral matter very much as fissure veins are. In comparing the two main types of these auriferous veins, and noting that they are not to be regarded as totally different in character, Coleman states that both must have been at one time open fissures through which water circulated; and it is not clear that he regards the formation of the veins in the absence of preëxisting open fissures as always possible.

[1] I prefer to restrict the true fissure veins to those occupying fault fissures and which are thus persistent in depth and characterized by slickensided walls and other selvage phenomena.

That the veins of both types are, in the main at least, of exogenous origin, that is, have been formed in the absence of preëxisting open fissures, by growth from the middle line outward, the growing vein mechanically displacing and not chemically replacing the wall-rock to make room for itself, is, to my mind, clearly indicated by the leading facts. Endogenous vein formation by incrustation on the walls of an actual or preëxisting open fissure, and the formation of vein-like replacements, are familiar conceptions; but true exogenous vein formation by segregation without replacement along a plane of weakness or in a merely potential fissure, the vein crowding back the walls as it grows and making room for itself in the solid rocks much as a root does in the soil, is a more novel idea. These are unquestionably the conditions under which many crystals and concretions are formed in the rocks; and they may safely be postulated for such isolated lenticular veins as those represented in Figure 6, since correlation with pure concretions, mechanically distorting and displacing the enclosing rock as they grow, is clearly preferable to the supposition that cavities of these forms were developed by plication and subsequently filled, and since replacement, metasomatic or otherwise, is ruled out by the perfect conformity of the veins and walls.

Among the normally distinguishing features of endogenous veins may be mentioned banding or comb structure and vugs, crystalline and botryoidal or mammillary mineral developments being directed inward (centripetal growth), and absence of isolated inclusions of the wall-rock save where there is evidence of a reopening of the fissure. Exogenous veins, on the other hand, should, normally, be distinguished by a massive structure, outward facing (centrifugal growth) of local and definite mineral developments, whether distinctly crystalline or not, and, very generally, by a median line or surface marking the primitive break and the inception of growth, and above all, by isolated inclusions of the wall-rock. The comb structure represented by the transversely fibrous or columnar structure of veins or veinlets of asbestus, chrysotile, satin spar, etc., is also believed to be extremely characteristic of, and essentially peculiar to, exogenous veins, while slickensided walls are entirely consistent with both modes of formation.

Judged by these criteria, the auriferous veins of Algoma, alike of the gash and sheeted types, are clearly exogenous, or at least the

truly endogenous examples must be of very exceptional occurrence; for they are massive, devoid of true comb-structure and vugs, and both isolated lenses and isolated inclusions of the wall-rock are highly characteristic.

They occur in a region of ancient rocks, perhaps the oldest on the continent, and a region also of extremely prolonged geological stability and subterranean inactivity, during which the erosion has unquestionably been enormous — to be measured by thousands of feet and probably by miles. We must logically, or according to the well-established precedents, refer the formation of the auriferous veins to the period of disturbance and metamorphism accompanying and following the folding of the Huronian strata and the development of the igneous complex, and to a time, therefore, anterior to the stupendous erosion which over wide areas has laid bare the ancient zone of flow and even the still deeper zone of fusion. Endogenous veins are clearly limited to the zone of fracture, where only open fissures can be formed and maintained; while exogenous veins may, in fact must, also characterize the deeper zone of flow, being co-extensive with every phase of the process of segregation directly dependent upon the molar or molecular movement of water in unfused rocks. In other words, the endogenous and true fissure veins of this region have, in general, disappeared with the ancient zone of fracture in which they originated; but the exogenous veins and replacements, comparable with pure concretions (concretions consisting wholly of the segregating mineral), which are, respectively, non-metasomatic and metasomatic in their relations to the enclosing formations, still persist, although individually of limited extent; and it is a necessary corollary of this view that their mineral contents, including their metallic values, have been derived from the immediate environment by some phase of the process known in general terms as lateral secretion.

That ore deposits of these types are now forming in this region (except possibly at a great depth), or have been formed in a geologically recent past, is improbable; but we may confidently assume that they belong to a remote past, and refer them to the deep region (not necessarily the zone of flow) where only is possible the greenstone alteration or chloritization of the granite, diorite, etc., which has supplied the quartz, carbonates, sulphides and other materials for the making of the veins.

SUCCESS IN LONG-DISTANCE POWER TRANSMISSION.

By F. A. C. PERRINE, D.Sc.

Read May 8, 1902.

Two elements make success in the long-distance electrical transmission of Power — Continuity and Regulation.

Perhaps these are the most important elements in any electrical undertaking and are the most obvious, but in attaining our ideal in long-distance work, there lurk pitfalls about these two elements unknown in other fields of the art.

From the outset in the original studies, and down the line to the last switch and insulation, lie unknown problems and problems known, but unsolved. It is the field of the thoughtful, independent engineer; the ground is stony and fit traveling only to the most resourceful.

Hydraulic development is one of the oldest arts, but the amount of human ignorance concerning hydraulic problems and conditions is appalling. Records may have been kept of the rivers whose waters are navigable, or whose rapids and falls are along the railroad lines, or in the manufacturing villages; but an investigation of the records available concerning important powers, even a few miles out of the regular lines of travel, would surprise you by their lack of accuracy and detail. Rainfall has been kept for generations for the benefit of agriculturalists, but the watershed of no one power stream of my acquaintance has ever been completely covered in its essential details by rainfall records.

The water available at any one point where the rainfall can be approximated will be estimated by different engineers from 15 per cent. to 70 per cent. of the total fall, and, apparently, each will be justified by engineering precedents. Such possible variations call for the exercise of caution, judgment, and experience in reaching conclusions.

Nor is there less uncertainty in regard to hydraulic construction. The present year has seen the failure of dams built by engineers of the highest reputation and at water pressures for which they were supposed safe. The flow in canals, ditches, flues, and pipes, are still subjects of

dispute, and errors in design are continually leading to serious results for the investor.

The one lesson to be drawn from all this being that, if continuity only means success, the studies in water-power location must be made with unusual care, and ample, though never ridiculous, margins allowed in the estimates.

Perhaps the day may come when the civil engineers will establish their empirical equation in hydraulics on a sure basis, but until that day is reached, and it is not near, the hydraulic engineer can never cease to be a student and most careful observer. In almost all cases the formulæ of engineering have a sound experimental foundation, but the error has actually been made of attempting to apply results obtained from the gaugings of the Mississippi to small rapid rivers, and even in some cases to lined canals and timber flumes, and an equal error committed of attempting to apply the results obtained from the flow of water in eight and ten inch pipes to great conduits and even open flumes. No smile should arise over the absurdity of these errors, for every state in the Union has instances of them, and no year in the past twenty but has seen them committed to the waste of thousands and hundreds of thousands of dollars. The records for the past five years at one of our most respected testing flumes are at fault on account of an altered pressure pipe and an uncorrected formula. Success comes to the cultured engineer who keeps close to his borrowed data and who solves for himself the unforeseen problems.

I mean by "Culture" what Matthew Arnold means when he says that culture is "A pursuit of our total perfection by means of getting to know, on all the matters which most concern us, the best which has been thought and said in the world."

Such an engineer with such an ideal will be a safe guide and his studies will establish on a firm basis our transmission undertaking.

But we are not concerned tonight with the errors of the civil engineer, or with his unsolved problems, so much as with what is new and with what is gradually getting to be known in electrical problems. While these are no more important nor more interesting than the hydraulic problems, they are attracting more attention, and perhaps it is in apology for our electrical ignorance that I call attention to hydraulic difficulties and ignorance. It is true that in no other art can so accurate calculations be made as in electrical engineering, but there are many facts and constants for our equations still unknown.

Even in the electrical art there is still an unknown, and I suppose that no one of us will live to see the discovery of even all the knowable our art contains. I am reminded now of the beautiful equation for the dynamo machine presented twenty years ago by Clausius, which contained a round dozen of unknown constants, and though we have since discovered the values of some of his constants, others have been found to have been variables, and we are still studying the problem and searching for the others.

Regulation in direct current work is a simple problem. Its laws are all known; at every lamp it is quite easy to predict the variation of voltage. The same laws control whether the lines be long or short. In his transmission plants Thury has met no new difficulties but has effected regulation at ten or twenty miles, just as he would have done had the delivery been made only in the next room to his dynamos. Resistance is always resistance and his dynamo characteristics remain unaltered by its increase. He deals only with current, voltage, and resistance and encounters problems simple even to Faraday, Henry, or Ohm.

It has been said very truly that the introduction of the alternating current raised the electrician above the rank of the artisan. With these currents he is no longer dealing with a few simple independent quantities connected by a primary rule, but must now consider many new elements, some constants and some variables. At every step capacities and self-inductions enter into the solution in complex and involved relations. Regulation can no longer be simply determined for each part of the system independently, and the regulation of the whole obtained by a consideration of the parts separately. Self-inductions and capacities affect each other, and the generators and lines, as well as the transformers and motors must be chosen or designed with this interdependent relation in view. A simple case illustrates this beautifully.

When the operation of the transmission supplying current to the city of Stockton, California, from the power plant at Mokelumne Hill, forty-six miles away, was first begun, a curious state of affairs was observed. In operation of the lines alone, the current required on the 2,200-volt side of the step-up transformers was sixty amperes; with the step-down transformers connected, the current fell to about fifty amperes, and when a 100 K. W. motor was put into operation the current was only fifty-five amperes. The transmission was designed

to be at 25,000 volts, but at the delivery end of the line the voltage was found to be 27,200 volts, and finally the motors consumed full current when running at no load and not much more when operating with full load applied.

I think that all will agree that here are problems enough for one small instance of the difficulties in the regulation of a long-distance transmission plant.

The lines in this case had been calculated for an eight-per cent. loss of energy for transmission of 1,000 K. W., and capacity played the most important part in these pranks of voltage and current. With the lines alone, the capacity was so great as to absorb an apparent energy of over 130 kilo-volt-amperes. This is reduced when the transformers are connected and some real energy begins to flow and the self-induction of the step-down transformers neutralizes some of the capacity of the line.

When the motors are connected, the real energy is increased and the self-inductances of the various parts of the system become more important.

The apparent rise of voltage along the line illustrates the interdependence of the whole system even more clearly, for it is only in a very small part due to the line itself, or rather, while due to the line, it occurs in the step-up transformers and not to any extent along the line.

It is not uncommon to attribute such an effect to resonance, which really only rarely becomes important; in reality, the regulation of the transformer produces this effect. With any transformer fed with a constant voltage at the primary, the secondary voltage falls with non-inductive load and falls still further if an inductive load is applied, but if the load is a capacity load, the secondary voltage rises instead of falls. In this case, therefore, the step-up transformers were delivering a ten-per cent. higher voltage to the line than their transformation ratio accounted for, which necessarily disturbed the regulation seriously. This trouble became even more serious when a heavy induction-motor was later operated by the same plant. The load on the motor accounted for a fall of potential amounting to four per cent., but the inductance produced a drop of ten per cent. at least; and, as the rise of voltage on the line at no load was as much as ten per cent., it may at once be seen that the voltage varied when the motor was connected from ten per cent. above normal to ten per cent. below, and thus a

variation exceeding twenty per cent. was produced in place of a variation of four per cent. for which the load allowed.

The effect upon the synchronous motor was also due to this ever troublesome capacity. The generators were giving nearly a true sine wave of E. M. F., while the motors were designed for a peaked wave, the line capacity so affected the generator wave that the difference between the motor and generator became great, and for part of each wave the motor was absorbing power, and for its remainder delivering power, so that always a great current was flowing.

In this particular case these troubles were corrected by the introduction of a heavy self-induction across the ends of the line which neutralized the line capacity; and this self-induction was used until an inductive load was connected in the shape of induction motors, which rendered the use of an extra self-induction unnecessary.

Whenever this arrangement can be thoroughly carried out and the capacities and self-inductions connected to any line be evenly balanced, the alternating problem can be reduced to the simplicity of the direct current problem and long algebraic equation abandoned in determination of the expectations.

This possibility is beginning to be appreciated and regulation attained in long-distance transmissions which could not otherwise be hoped for.

There is little service which calls for absolutely continuous power. Most plants, which are said to be operating "day and night," shut down at meal times and are rarely operated more than twenty hours of the twenty-four, and even where the operation is continuous through the day, there is a cessation of operation on Sundays and holidays. For this reason, it is difficult to satisfactorily balance the capacity of the line against the inductance of any load. The line capacity current is constant and is continuously in evidence so long as the voltage is applied to the line; indeed, it varies only with the voltage and periodicity. Its importance increases as the load diminishes, for when the load is heavy it is not only completely overcome by inductance, but also is rendered unimportant by reason of the presence of a large current in phase with the electro-motive-force. During the time when all loads are diminished, the disturbance of regulation by reason of the presence of a line capacity-current is most apparent, and consequently the counteraction of that capacity effect, by a heavily inductive load which is off during these same periods, is a remedy which

only accentuates the disease. This was illustrated in the case described above where a severe induction load depressed the voltage and its removal allowed the full rise due to line capacity.

In the case of the Standard Electric Company's transmission and that of the Bay Counties Power Company, where transmission of approximately 150 miles at 50,000 volts and 60 cycles are undertaken, the charging current is approximately 40 amperes; or, in other words, the line requires the full capacity of a 2,000 K. W. machine for charging it as a condenser. The complete neutralization of this great capacity-effect would require the continuous working of something in excess of 5,000 K. W. of induction motors with average normal power factors.

Up to the present time no load on the Bay Counties lines has ever been applied which is capable of neutralizing this capacity effect. The Power House is practically unable to have much knowledge of the loads actually applied on the lines except by observing the wattmeters or the wheel nozzles, since the current from no load up to a load of several thousand kilowatts remains practically constant. As there are many branching lines supplied from this system, it is impossible to operate other than with constant E. M. F. at the dynamos, and the regulation of the long lines is affected by the capacity, which influences everything from the step-up transformers to the last motor.

In order to overcome the troubles due to this source, the Bay Counties Company have arranged to place upon their lines impedance coils capable of practically neutralizing the entire capacity of their long lines; and, as soon as these are installed, one of their greatest difficulties in obtaining satisfactory regulation will be removed. With the capacity of the line neutralized, it then becomes necessary to keep the load as nearly non-inductive as possible by continuous care in the balance of synchronous against induction motors in the operation of loads.

It is a great mistake for an engineer to become an advocate of one motor to the exclusion of the other type. Both synchronous and induction motors have their spheres of usefulness, and practically every long-distance transmission demands for its satisfactory regulation the use of both types. Where the powers are small and the loads easily started, the simplicity of the induction motor and ease of installation renders it especially suitable, but as their numbers increase, the effect of the lagging currents they absorb becomes important in disturbing regulation, so that it soon becomes necessary to neutralize this lagging

current either by the introduction of condensers or of synchronous motors.

With a synchronous motor it is possible to counteract the effect of large and variable inductive loads, and, when they are installed in the sub-station at the delivery end of the line, a ready and satisfactory means of controlling the voltage, whether it is disturbed by inductive or non-inductive loads, is provided. Practice with these machines indicates, however, that they are still very useful if installed at points out of control of the sub-station attendants, provided only that their power of controlling voltage be not used maliciously, since they are generally most useful when the exciting current is adjusted to a minimum; a condition for which instructions are easily issued and of which the reasonableness appeals to the most ignorant attendant.

Much has been written and said of the resonant condition of a line, but up to the present time practice in the installation of plants and connection of motors, transformers, and other devices has not led to important interference from this source. What is called the resonant line, or distortionless line, can be obtained in two ways, either by the connection of capacities and inductances in series, or by their connection in parallel. In the first method of connection there is a very serious rise of potential at the respective terminals of the capacities and inductances, but with the parallel connection no such rise is found. The series connection of capacities and inductances is rarely used and, principally, on account of the difficulties likely to be encountered on account of these phenomena. The exact balance of inductances against the capacity of a line fifty or one hundred miles long, capable of supplying some thousands of kilowatts, would result in a voltage rise of not less than ten times the value of the normal; and, when one considers that this normal may be anywhere between twenty-five and fifty thousand volts, it is easily seen that such resonance may be formidable. Such an effect is sometimes actually encountered in the operation of switches on lines where considerable cable lengths are used in feeding highly inductive apparatus. In a recent case an arc within a partially broken cable feeding rotary transformers, by raising the periodicity and producing apparently a resonant condition, caused the effect of most violent short circuits, blowing fuses with great violence and destroying both fuse holders and switches, currents appearing entirely beyond the capacity of the generating apparatus to maintain. When the proper inductances are connected in parallel, however, their action

is beneficial to regulation and easy to handle, as has already been explained.

What has so far been said covers fairly well the question of purely electrical problems of regulation, and to call your attention to the essentials of the problem, it only needs the summary that regulation in a long distance plant is attained by —

First. A wide possible voltage variation in the generators, with generators and transformers both designed, not for good non-inductive load regulation, but for good inductive load regulation.

Second. Lines designed for the smallest possible capacity current without reference to any attempt at balancing the line capacity against the load lag.

Third. The use of reactance coils for balancing the constant capacity of the line.

Fourth. The use of synchronous motors as a variable capacity for balancing the variable inductance of the line and the inductive motor load.

Of the elements of design which involve most nearly the question of continuity, supreme importance attaches to the insulators, the switches, and the lightning arresters.

To be sure, the entire problem of continuity is also the whole problem of design. Everything must be built to withstand the strains, and the methods of accomplishing this are neither easy nor obvious; many of the well established methods of conveying and controlling water must be abandoned in favor of those which are more enduring. Open ditches and flumes are giving way to enclosed and lined trenches and tunnels.

For station structures, the extreme of strength and fire-proof character must be resorted to and the whole transmission line must be built for the greatest permanence. Spans are shortened even in some cases to only one third of that used in telegraph and telephone construction. Heavier poles are employed, most carefully set, and braced against even the slightest increase of strain. Rights of way are secured and cleared so that these transmission lines stand up against the storms and fires that lay waste the countryside.

But returning now to the purely electrical part of the problem with which we are most directly concerned for the present, and directing our attention to the insulators, which present the first and most difficult problem, we find at the outset a royal battle waged between

the insulating properties of glass and porcelain. In this country glass has for many years been the king of the insulators; abroad for an equal period of time, porcelain has remained preëminent.

For potentials up to about 25,000 volts, where an insulator of seven inches diameter is sufficient, there seems not much reason for the employment of anything except glass, unless the lines reach a size above half an inch in diameter, when its strength is deficient. For these lower potentials and lighter lines the good properties of glass are easily summarized. In the first place, it is cheaper by far; while not so strong as porcelain, it is, as has already been said, sufficiently strong for the purpose; its inspection is easier and surer, since it requires only a visual examination and a few taps of a hammer to ascertain its soundness in place of the tedious high potential test necessary in the examination of porcelain. But having said this much, all has been said that is possible in favor of glass.

It is not true that glass is a better insulator than sound porcelain, nor is its surface so good in damp weather against surface leakage. The mechanical strength does not equal one half that of porcelain, as has been proved by a long series of mechanical tests performed by dropping a steel ball from a height upon insulators of the two materials; temperature also affects it much more than porcelain and it is often found to crack simply from the effect of extreme changes of temperature.

The insulators for high voltage lines must be very large, for the reason that with a striking distance through the air of from four to six inches, great gaps must be provided in the path of the current. But above all a great creeping distance is essential.

Insulators that are a perfect protection in the hardest rains fail utterly in clear weather when covered by smoke and soot, which allow small amounts of currents to escape and char away the pins or cross-arms. The compound insulator is very attractive, for the reason that it is easier and cheaper to make and handle two or three small parts rather than one large one, and in some cases the additional advantage has been sought of securing the good properties of both glass and porcelain by making parts of an insulator of each; but there is both a theoretical and practical fallacy in this type of design. In the first place, no dielectric is so strong as one entirely homogeneous. As the stress is passed from glass to porcelain, or *vice versa*, at the two surfaces the stress piles up as it were, and it is better if one believes in glass to rely upon it; or, on the other hand, to show faith in the porcelain used by employing it entirely.

From the pure standpoint of practice the insulator which must be cemented together is troublesome. The shrinking of the mastic leaves voids and produces strains; if cement be used, its slowness in hardening delays the work and requires large areas for setting out to harden any considerable number of insulators. The one other mastic in use is molten sulphur, which, while better as a mastic than cement, is still more liable to crack the insulators while they are being joined, and finally when exposed to light and air, frequently decomposes on the surface, producing sulphuric acid; and, finally, when current creeps over its surface following the acid, perhaps, catches fire and often results in the rupture and fall of the insulator. The insulator which will be entirely satisfactory for this use has not yet been found, but the diligent search made for it by some of our best engineers is resulting in its definition and will probably soon result in its production, not as an inspiration for a patent, but as a careful engineering conception.

Switching and switching devices are most important and most difficult. Step by step the switch has improved in the electrical art from the simple commutator used by Faraday and Henry, which generally consisted of copper wire dipping into mercury cups; until, as you know, up to the present time a great amount of ingenuity devoted to the study has resulted in the production of elaborate and beautiful electrical and mechanical mechanisms. Switching for high powers, whether at low potential and great currents or at high potential and small currents, seems to be an altogether different art from switching where the spark that may follow can do but little destruction, and its heat is readily dissipated. Almost all low-power switches are designed with the idea that a spark may occur at the break, but as the energy of the spark is small, the mass of the switch will carry away the heat, or the portion of the switch destroyed will be inconsiderable and may be easily replaced; but as soon as it becomes impossible to contemplate the destruction of a portion of the switch in the rupture of the current, switching becomes impossible by such simple devices, the whole art of switching is changed, and designs along entirely new lines must be produced. The great pneumatically controlled switches in the large power stations of New York and Boston exemplify this principle. Hardly anything in switching for small currents is similar. They are not simple switches, giants in size, but their method of operation is based upon a new principle and they involve in their construction much ingenuity and engineering study. As even the great switches with

which you are familiar operate only on lines where the potential is comparatively low, the principles of high voltage switching need to be particularly called to your mind. In any high voltage transmission system as, indeed, in any transmission system, the lines and loads contain not only resistance but also capacity and self-induction. The current flowing depends upon the E. M. F., the frequency, and the constants of the circuit in an involved relation. A change in any one of these constants involves changes in the resultant current. A satisfactory switch must leave the circuit undisturbed as far as possible, and must introduce no new difficulties by reason of changed values of the constants. Furthermore, if a switch could be built which would simply instantaneously stop the current, the effect would be an immediate and great rise of potential due to the sudden fall of current through the capacities and self-inductions of the system. The open air switch which, in its essential particulars, is simply that of the ordinary knife, or snap, switch as used for low potential, draws an arc at almost full current value, which arc, being rapidly oscillating in character, effects an increase of the periodicity on the line and thereby brings about great potential rise. We see, therefore, that a switch to be satisfactory on a high potential circuit must neither rupture the circuit too suddenly nor draw an oscillating arc. With ordinary switches in mind, the rupturing of a circuit with these requirements in view seems to be almost an impossibility; and, were there no means of introducing into the path of the switch blade high resistances which gradually reduce the current value, and finally allow the stoppage of the flow of energy after it has become small in amount, we would have to provide in insulation of our long-distance high-potential lines, materials which would withstand voltages from two to ten times the normal voltage. Of course no wire wound or carbon rheostat can be introduced into these switches, and the only resistances that are available are liquid and gaseous resistances. It is almost too soon to say that any particular type of switch has resulted from the experiments of the past two years which will be considered for all years to come the final type; but the successful work that has been done is along the line, as I have said, in the introduction of liquid and gaseous resistances. These resistances are the only ones that are high enough in value to be enclosed in the small space of a switch and which do not introduce additional complexities into the station system. The gaseous resistance is obtained in the oil switch by the carbonization of the oil as

the switches open ; provided there be a sufficient amount of oil above the switch contact, so that as the circuit is opened the production of gas does not blow the oil out of the switch, a very satisfactory type of switch is produced, and one which our present experiments seem to indicate, will gradually cause the rupture of a circuit without the production of excessive rise of potential.

The vaporization of metallic oxides has also been successfully used for this purpose, but when these oxides are vaporized in the switch, it is difficult to retain them within the switch and they scatter their dust through the whole station. The liquid switch is one of the comparatively new developments, and seems in many respects to be the best of all, since the liquid is always prepared as a resistance under all circumstances and does not depend upon the energy of the current to make it the imperfect conductor required. Therefore, the disadvantage of the occasional failure of the switch from too rapid or too slow production of the vapor, as is the case with the vapor switch, is avoided. An incident in the difficulty of obtaining the proper switch for high potential working is the difficulty of insulating its moving parts and operating mechanism. Insulation materials of high dielectric strength are not ordinarily satisfactory mechanical materials. They are brittle, they are hard to work, often impossible to work, and are not readily obtained in exact form. Hence, many a switch that is entirely satisfactory from an electrical standpoint proves to be a failure on account of the difficulties with its mechanical support.

Lightning protection for long distance transmission lines is, as one can readily see by the slightest consideration, of the utmost importance. Where a line passes over one hundred or two hundred miles of open country, dipping into the valley and rising to the tops of mountains, all sorts of weather conditions must be constantly encountered. A thunder storm of small area is, in summer time, a matter of frequent occurrence ; and when we consider that, as the line stretches way across the country, many small areas of atmospheric disturbances may be and must be daily encountered. To be sure, as you all know, the lightning protection of a transmission line is not against strokes of lightning, but against the induced currents in the line by reason of neighboring lightning strokes. Furthermore, any transmission line must be protected against variation of atmospheric potential in clear weather. Experiments have shown that changes of elevation by a few hundred feet sometimes produce potential variation of many thousand

volts and these phenomena must be encountered and handled by the transmission engineer. One of the most effective devices that has been used is the erection of a continuous lightning rod along the entire line. For this purpose barbed wire, grounded at the poles, has been employed. In some instances this has given trouble on account of the fact that the barbed wire used was inferior in quality and erected with unsafe strains; but when the wire has been carefully erected, it is not necessarily a disturbance to the system, but a means of discharging the wire when the atmospheric potential varies and as induced current surgings are set up by neighboring lightning discharges. It is not by any means a settled principle with transmission engineers to install this barbed wire, though in the South where the lightning disturbances are most frequent, it may be said that almost every line is erected with this protective device. Where it is not used, reliance is placed altogether on the lightning arresters, through which the line may be grounded, these being installed either at the ends of the line or at the ends and at intermediate points; and even where barbed wire is employed, such additional protective devices are absolutely essential, since the circuits themselves can not be entirely freed from induced charges by any means, except enclosing them within a grounded metallic sheath, and this construction would, of course, be absurd and unmechanical. No mechanically operated lightning arresters are ever used for this purpose in alternating long-distance transmission work, the devices employed being either of the non-arcing metal type or of a type where the air current produced by an incipient arc between the parts of the arresters blows out the arc before the machine current has an opportunity of following and affecting destruction. In some devices the combination of these two effects is to be found. These constructions of lightning arrester are old and well tried at lower potentials, but it is found that as the potentials rise, the laws which seem to govern their action change materially, and no step has been made in raising the voltage without requiring material modifications of the rules for the installation of lightning arresters. Two thousand volt calculations have been found to fail altogether on 10,000-volt lines and these again to fail when the potential was raised to 25,000 volts, and finally with the doubling of that potential, the previously ascertained rules seem to be altogether thrown to the winds, and a new system of lightning protection made necessary. The final form in which this will be received by the engineering world has not as yet

been established, but the experiments which have been tried up to the present time, seem to indicate that a resistance or an impedance, or, resistance and impedance combined, must be installed in the path of the ordinary lightning arrester in order to enable the induced charges on the line to pass away readily without noticeably increasing the effective line potential and without grounding and short-circuiting the wires. The handling of this problem has been one of the last to be solved and one of the most troublesome questions involved in the engineering of transmission lines.

I have this evening been endeavoring to call to your minds the problems met by the transmission engineer and the difficulties he has to solve, which are not presented to his brethren in handling ordinary electric lines and currents; and though I may have given you the impression that there are many questions yet to be solved in order to bring about success in long distance transmission, when success is measured by practically perfect regulation and practically absolute continuity, still it would be incorrect for me to have given you the impression that the engineers are still groping in the dark for a solution of their problems for the attainment of success as measured by the criteria I have laid down. The limitations I have placed upon the solutions that have been already applied relate only to the question as to whether present solutions are in their final form, and it would be a wonder of the world if, with long-distance transmission beginning in 1894, with a small plant at Portland, Maine, and a small plant at Housatonic, Massachusetts, the solution that had been reached in 1902 for the problem as extended to 200 miles were in the final form; and if I have given you tonight some of the inside history of the difficulties to be encountered by transmission engineers, you would be doing me and those engineers an injustice to believe that I have been doing anything excepting calling to your attention the difficulties that have been met and the construction which still remains to be improved, while at the same time announcing the triumph in practically and successfully transmitting power up to 215 miles, and practically and successfully building at least, two plants for the permanent distribution of energy at distances in each plant exceeding 150 miles.

THE RECORDED HISTORY OF THE MEMBERS OF THE ARGON GROUP.[1]

By HENRY P. TALBOT.

In his work "On Air," at the opening of the chapter in which he describes the isolation of the gas which we now recognize as oxygen, Priestley writes as follows: "The contents of this section will furnish a very striking illustration of the truth of a remark which I have more than once made in my philosophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philosophical investigations, viz: that more is owing to what we call chance, that is, philosophically speaking, to the observation of events arising from unknown causes, than to any proper design or preconceived theory, in this business."

In 1774 Priestley believed the atmosphere to be a simple, elementary substance, as little destructible or alterable, except as to the admixture of phlogiston, as water was then supposed to be. We learn from Priestley's interesting writings that it was the chance purchase of a burning-lens, and the accidental presence of a lighted candle at an opportune moment which led to the "events," the "unknown cause" of which proved to be the "dephlogisticated air" obtained when mercuric oxide was heated by the concentration of the sun's rays with the aid of the lens. It was only after a long series of experiments that the new gas was recognized to be a constituent, but not the whole, of the atmosphere. The discovery of oxygen was, therefore, not the result of a systematic search.

In the interval of a little over a century between 1774 and 1894 much progress had been made in the accumulation of knowledge regarding the atmosphere, and at the latter date its constituents were regarded as qualitatively and quantitatively established beyond reasonable doubt. But in 1893 one of the "events" referred to by Priestley

[1] Read before the Northeastern Section of the American Chemical Society, February 27, 1902.

occurred, the "unknown cause" of which it became necessary to establish, and it was during the progress of the resulting investigation, as we shall see, that the element which gives its name to the argon group was discovered.

We all remember well the incredulity with which the announcement of this discovery was first met. Hardly since the days of Berzelius, Gay-Lussac, and Davy had there been new elements discovered, save those of rare occurrence in relatively obscure minerals. We were asked all at once to believe that the measurements of the constituents of the atmosphere, made during a century of investigation, had been so faulty that the presence of an element to the amount of about $\frac{1}{100}$ of the atmosphere had been wholly overlooked, and that a substance which we had been taking into our lungs to the amount of 20 liters in each 24 hours, had received no recognition. It is small wonder that chemists everywhere were disposed to be skeptical, and to hold themselves ready to assume a position of general superiority in case the new element proved to be elusive. But the statements then made by Lord Rayleigh and Professor Ramsay have been fully substantiated, and although the discovery of argon was a sort of a by-product from another investigation, we shall see that the discovery of the other elements of this group (except helium) was the result of systematic search, and that the whole investigation was painstaking in the highest degree, and was deservedly rewarded.

The story of the discovery of argon in the atmosphere, which is so pleasantly told in Professor Ramsay's book on the "Gases of the Atmosphere" is doubtless familiar to most of you, at least in its general outlines, but a brief review of this early work will, I feel sure, not be out of place.

In 1893, Lord Rayleigh began a series of determinations of the densities of the principal gases, and in the course of these experiments he made determinations of the density of nitrogen derived from the atmosphere, and of nitrogen derived from such bodies as nitric and nitrous oxides, and ammonium nitrite. The conditions of experimentation were such that he considered his results to be accurate within one ten-thousandth of their value, yet he found that, while the "nitrogen" from the atmosphere contained in his globe weighed on an average 2.3101 grams, the same volume of nitrogen liberated from its compounds weighed only 2.2990 grams, a difference of 11 mgm. in 2 grams, or about one part in two hundred. To confirm these

results subsequent determinations were made using nitrogen prepared from urea, and from ammonium nitrite, without passing the nitrogen over heated metal, in order to eliminate the possibility that the lessening of the density of the chemical nitrogen was in some way due to this heating which was a part of other methods employed; but the figures obtained were in entire agreement with those already noted, and it was further found that if the nitrogen of the atmosphere was absorbed by magnesium at a red heat, and the resulting magnesium nitride afterward decomposed, the nitrogen thus obtained agreed in weight with the chemical nitrogen from its compounds. Beyond any doubt, then, a real difference in density existed, and an explanation must be found. Not being able to supply one himself, Lord Rayleigh asked for the assistance of the chemical fraternity in the solution of his problem.

Suggestions were not lacking! It was said that "the chemical nitrogen contained hydrogen." But it was found that a mixture of hydrogen and atmospheric nitrogen could be separated and the specifically heavier nitrogen isolated, as before. Again, it was suggested that, "the nitrogen had in part polymerized after the fashion of ozone, and that atmospheric nitrogen contained molecules N_3"; or "the chemical nitrogen contained molecules of less size than those with a symbol N_2." Both these suppositions were rendered improbable by the fact that after being subjected to the action of the silent electric discharge, the densities of the two gases differed exactly as they did originally. Moreover, if the nitrogen molecules were dissociated in case of the lighter chemical nitrogen they might reasonably be expected to recombine on long standing; but after eight months the lighter chemical nitrogen persistently retained its lesser gravity.

In short, the probability of the presence in these gases of any known element other than nitrogen, or of the presence of polymeric forms of the element nitrogen itself, was gradually eliminated. There remained, then, only the supposition that there was either an unknown lighter element in the chemical nitrogen, or an unknown heavier element in the atmospheric nitrogen, the latter despite the vast amount of experimentation upon the atmosphere in which chemical literature abounds. The problem was first attacked from the latter standpoint, and Professor Ramsay joined forces with Lord Rayleigh.

The classic experiment of Cavendish was repeated. The atmospheric nitrogen was mixed with oxygen, and subjected to the action

of the electric discharge for some time, the product of the combination being absorbed by caustic alkali. A small unabsorbed residue was always obtained which was proportional to the quantity of air operated upon. It is interesting to note that Cavendish reached exactly this point in his experiments upon nitrogen a century ago. Cavendish remarks that since the small bubble remaining is less than $\frac{1}{120}$ of the bulk of the air used "if there is any part of the phlogisticated air of our atmosphere which differs from the rest and cannot be reduced to nitrous acid, we may safely conclude that it is not more than $\frac{1}{120}$ part of the whole." This small residue obtained by Cavendish did, in all probability, represent what we now recognize to be argon, with minute amounts of other members of the argon group. The determination was nearly quantitatively accurate, and is a striking evidence of the skill of this remarkable experimenter.

Without entering into details of the procedure the results may be summarized in the statement that the residual gas, obtained by long-continued treatment by the Cavendish method, and by another wholly distinct procedure, viz: the repeated circulation of the atmospheric nitrogen over red hot magnesium for a number of days, showed a density of about 19.9 to 20, as compared with hydrogen, and a spectrum in which that of nitrogen was not to be detected, and which was new, characteristic, and uniform in different samples of the gas.

In order to show that this increase in density was not due to any change which the atmospheric nitrogen had undergone during heating, the processes of atmolysis were also employed to concentrate the heavier gas. A number of stems of church-warden tobacco pipes — about twelve — were connected in series, and these were enclosed in a large glass tube, the space between the pipe-stems and the tube being exhausted. At the same time a current, amounting to about 2 per cent. of the total air entering the mouth of the pipe-stems, was drawn out at the other end. It is evident that, in accordance with the law of Graham, under these conditions the gas which failed to be drawn through the pores of the pipe-stems (that is, the portion collected at the open end), should have greater density than that which passes into the vacuous space, provided the atmosphere contained a heavier constituent. In point of fact, the results confirmed the expectations. The air which failed to diffuse through the pores of the pipe-stems had increased materially in density, and after removal of the other constituents (oxygen, water, carbonic acid, and ammonia) 'the residue was distinctly denser than atmospheric nitrogen.

It was further noted that this heavier residue exhibited a relatively high solubility in water, now known to be about 4 volumes in 100 of water, at 12° C., or about the same as oxygen. Since this is about two and one-half times the solubility of nitrogen the gas expelled by boiling from water which has been exposed to the atmosphere should be relatively dense, or rich in the heavier constituent if atmospheric "nitrogen" is a mixture of these gases. This was found to be true, an increase of 24 milligrams being noted in a weight of 2.3221 grams.

At last, in 1895, Lord Rayleigh and Professor Ramsay felt justified in announcing the discovery of a new element in the atmosphere, to which, on account of its inert character, the name of *argon* was given. The announcement was made at a meeting of the Royal Society in London, on January 31, 1895.

The quantity of argon seemed to be about 1 per cent. of the atmospheric nitrogen, or 0.9 per cent. of the atmosphere. Assuming that the density of argon is 20 — a point to be discussed later — the greater density of atmospheric nitrogen as compared with chemical nitrogen would correspond to this percentage. Later investigations show that argon forms about $\frac{1}{120}$ of the atmosphere by weight and $\frac{1}{125}$ by volume, and that the amount is practically constant. Kellas[1] has claimed to find a somewhat larger amount of argon in respired than in normal air. It has been found in one specimen of meteoric iron, in the waters of a number of mineral springs situated in the Pyrennes, in Wildbad, in the Black Forest, and at Bath, England, and in the gas which escapes in bubbles at one or two springs in France. It has also been found in the gases occluded in rock salt, and as a constituent of fire-damp at Rochbelle.

It has not yet been found to be a constituent of either animal or vegetable matter, at least as far as these are typified by mice and peas. It has been found to be a part of the gas found in the air bladder of fishes.

Argon, as originally derived from the atmosphere, did contain other gases, as will presently be shown, but after the removal of these gases, the residue seems to be an elementary substance, and one not previously known, although articles still appear in which it is claimed that argon is a polymer of nitrogen. This idea finds confirmation only in the fact that many lines of the spectra of the two gases lie very near together, and the fact that if this were true, argon would

[1] Proc. Roy. Soc., **59**, 66.

possibly fit better into the periodic system. On the other hand, in no case has it been possible to detect anything corresponding to argon in nitrogen from chemical sources; the density, as will be seen, does not correspond with accuracy to N_3; and the existence of other members of a group in which argon seems to have a logical position appears to confirm its independent existence.

The chemical properties of argon seem capable of expression in a most concise form. It has no chemical properties. In one of the earliest papers, the following statement was made regarding the attempt of Rayleigh and Ramsay to discover some reaction into which argon would enter: "Argon does not combine with oxygen in presence of alkali under the influence of the electric discharge, nor with hydrogen in presence of acid or alkali, nor when sparked; nor with chlorine dry or moist, nor when sparked; nor with phosphorus at a bright red heat; nor with sulphur. Tellurium may be distilled in it; also sodium and potassium. Red hot sodium peroxide has no effect. Persulphides of sodium and calcium have no effect at a red heat. Platinum sponge does not absorb it. Aqua regia, bromine water, bromine and alkali, and potassium permanganate are all without influence. Mixtures of metallic sodium and silica, or sodium and boric acid are without influence, hence also nascent silicon and boron.

Moissan [1] has experimented with argon and fluorine at high and low temperatures and finds no action, nor is there any action upon titanium, boron, lithium, or uranium at high temperatures.

Ramsay at one time thought that he had effected a combination with carbon at the temperature of the electric arc, but it subsequently proved that the expansion observed was due to the carbonic acid occluded on the carbon rods. He found no action with carbon tetrachloride.

The literature contains accounts of a few experiments which have, temporarily at least, been assumed to indicate that argon had been persuaded to combine with certain elements or radicals. These include the considerable series of experiments made by Berthelot, [2] in which he mixed argon with the vapor of benzene, carbon bisulphide, and a number of other substances belonging to both the fat and aromatic series of organic compounds, and subjected the mixture to prolonged action

[1] *C. R.*, 120, 966.

[2] Described in *C. R.*, 120, 121 and 129.

of the silent electric discharge. In the case of benzene he observed a diminution of volume of the gas and the formation of a brown substance, which exhibited a brilliant green fluorescence and spectrum. The quantity formed was too small for further investigation, and the evidence of actual combination is not conclusive. Ramsay was unable to obtain parallel results, and other investigators have pointed out that the green spectrum is very likely due to the presence of mercury vapor from the pump. Berthelot has later contended that the compound producing the fluorescence contains mercury and the phenyl group, but here, again, his experiments with the phenyl-mercury compounds are not wholly convincing. It should be added, however, that he finds something of a uniformity of behavior among a number of the benzene compounds which he examined, while the members of the methane series are uninfluenced by the same treatment.

Troost and Ouvrard [1] made the statement that they had effected a combination of argon and magnesium. This is based upon their observation that if magnesium is introduced into a vacuum tube containing a very small amount of argon, the spectrum of argon gradually disappears, and they conclude that it is absorbed by the magnesium, or combines with its vapor. It is not impossible that if platinum electrodes were used here, the platinum, rather than the magnesium, absorbed the small quantity of the gas on its surface.

Villard [2] finds that under a compression of 150 atmospheres, in the presence of water at 0°C., argon forms a crystalline hydrate, which readily dissociates at a pressure of 105 atmospheres. The presence of argon in the crystals was not proven, but the author judges from analogous behavior of nitrogen and oxygen that they contain argon.

The complete lack of chemical data concerning this unique element lends special interest to the study of its physical characteristics. Of these three are of special importance — its spectrum, its specific gravity, and the atomicity of its molecule.

The spectrum of argon, at 3 mm. pressure, shows a great number of lines over a very wide field. Two red lines less refrangible than those of hydrogen or lithium, a yellow line more refrangible than sodium, a group of five green lines, a blue line, and a group of five violet lines make up the principal part of the collection. The spectrum undergoes

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[1] *C. R.*, 121, 394.

[2] *C. R.*, 123, 377.

a striking change when the nature of the discharge is altered. With an intermittent discharge the glow is red, with an oscillatory discharge the glow is bright blue, and many new green and blue lines appear. With a Tesla discharge the glow becomes nearly white, but the spectrum is similar to the blue spectrum, while by passing an alternating current of low voltage through the primary circuit of the induction coil, a peculiar spectrum is produced in which certain red lines are particularly pronounced. Crookes states that the spectrum from the blue glow shows 119 lines, while that from the red glow shows 80 lines, and 26 of these appear to be common to both spectra.

These variations have been made the basis of arguments against the elementary character of argon, but it should be noted that nitrogen also gives a double spectrum according to the character of the induction current.

Hartley [1] points out that since all known compounds are decomposed at the temperature of the electric spark and since its spectrum shows no familiar lines, argon, if a compound, must be made up of elements all of which are as yet unknown.

The density of argon is of the greatest interest, and has been most carefully studied. Ramsay has made repeated determinations of the density of the gas after repeated purification, the separation from the other members of the group being so complete that no evidence of their presence was indicated by the spectroscope. These determinations were accomplished by direct weighings in globes of known capacity, varying from 30 c.c. to 160 c.c. The best results seem to point to 19.96 as being the true value for the density of the gas, which corresponds to a molecular weight of 39.92 (O = 16).

What, then, is the atomic weight of argon? This question assumes prime importance, since upon it depends the relation of this new element to the other members of the periodic system of Mendelejeff. It was early determined that the ratio of the specific heat of argon at constant pressure, to the specific heat at constant volume, is 1.61 to 1.65. As is well known, this is the ratio which, from theoretical considerations, is assumed to correspond to a monatomic molecule, in which the whole of the heat-energy is converted into translatory motion. Repeated experiments have confirmed this value for the ratio of the specific heats, and the conclusion seems to be — if the premises are

[1] *Chem. News*, 74 209.

sound — that argon is a monatomic gas, that its molecular weight and atomic weight coincide, and that this value is 39.92. Incidentally this evidence may be said to point to the elementary character of argon, since it is improbable that a compound substance would exhibit this behavior with respect to the utilization of all the energy for translatory motion. It does not, however, necessarily preclude the presence in the argon of other monatomic elementary molecules. These points will be touched upon again.

It is of interest to note that lithium will remove nitrogen from argon with rapidity, and that metallic calcium formed from a mixture of anhydrous calcium oxide and metallic magnesium also removes the nitrogen with greater promptness than magnesium alone. The lime must be as dry as possible, otherwise explosions ensue, and even under great precautions some hydrogen is usually mixed with the argon, due to the action of the traces of moisture on the magnesium. This process is used in the preparation of argon in quantity.

The other important properties of argon are given in the table on page 227.

Helium.

In 1868, while examining the spectrum of the chromosphere during an eclipse, Jannsen noted a brilliant yellow line which was close to, but not identical with, the well-known D-line of sodium. This he designated as the D_3-line, and in the same year Lockyer assigned the name "Helium" to the hypothetical element of which this line was characteristic. Since that date the line has often been observed without the conditions which accompany an eclipse, by projecting the image of the edge of the sun on the slit of a spectroscope of wide dispersion. The D_3-line is also found in the spectrum of the corona and in that of some of the fixed stars and nebulas.

In 1882, Palmieri stated that he had discovered helium in a substance which is found with the lava of Vesuvius; but it appears that he did not attempt to isolate the helium, merely observing the spectrum of some body which gave a line corresponding to the D_3-line. It is not improbable that he was correct in his suspicions, although his proof is incomplete. Such was the knowledge of helium up to 1895.

After the discovery of argon, and the announcement of its inert character, Mr. Myers, the mineralogist of the British Museum, wrote to

Professor Ramsay calling his attention to the fact that cleveite, (a uranate of lead, thorium and uranium) had been shown by Hillebrand to contain nitrogen, and offering the suggestion that this mineral might also contain argon, or that some of its constituents might prove to be capable of entering into reaction with the inert gas. The suggestion was adopted by Professor Ramsay, and, after some months of delay as a result of his investigations in other branches of research, he obtained a quantity of cleveite, and examined the gas which was liberated from it by treatment with sulphuric acid, subjecting it to the methods of purification from nitrogen already spoken of in connection with argon. When subjected to spectrum analysis the residual gas showed argon, to be sure, but it also showed a new series of lines in the red, green, blue, and violet, and notably a brilliant yellow line. At this time, however, Ramsay was employing magnesium electrodes in his vacuum tubes, in the hope of thus absorbing any residue of nitrogen, and as the metallic magnesium had been reduced from its compounds by means of sodium, he attributed the yellow line to a small amount of the latter metal which had been volatilized. But on careful examination he was surprised to find that he had two bright yellow lines very close together, — but still distinct. He soon determined to send the gas to Crookes for examination, and Crookes at once reported by telegraph that helium could no longer be counted as among the extra-terrestrial elements. The yellow line was the D_3-line with a wave-length of 5875.9. The D_1-line is located at 5896.1 on the same scale. Experiments then made gave 3.89 as the maximum density of the gas, and the ratio of the specific heats was found to approximate 1.66. The gas is, then, as was to be expected from its existence in the chromosphere, very light, and it appears to be monatomic.

All the minerals in which Hillebrand had found nitrogen contained uranium. Ramsay examined not alone such minerals, but a great many substances of varying composition, by heating them in an exhausted tube, taking care to prevent the admission of carbonic acid or moisture. It was incidentally found that many minerals gave off hydrogen, and it was shown quantitatively that this hydrogen results from the action of water on oxidizable constituents of the minerals, such as ferrous compounds.

The helium may be liberated from the minerals by heat alone, by treatment with sulphuric acid, or by treatment with acid potassium sulphate. Under the influence of heat alone, the gas begins to be evolved at about 200°, and the evolution becomes rapid at a red heat,

but the total amount obtained is approximately half that liberated by sulphuric acid. The decomposition by means of acid often requires a number of days for completion; that by acid sulphate is more rapid, but the decomposition of the mineral is never complete.

Ramsay preferred the use of heat alone, and his procedure was the following: The tube containing the mineral was exhausted as completely as possible before heating. The gas evolved from the mineral was pumped out of the tube and was first passed through a solution of caustic soda to remove carbonic acid, then mixed with oxygen and sparked to remove hydrogen and hydrocarbons, then treated again with caustic soda; finally the gas was mixed with oxygen and exposed for several hours to an electric discharge to remove nitrogen. The residue is argon and helium with some oxygen, which is afterward removed by phosphorus. For the examination of the spectrum a Plücker tube was exhausted and one or two bubbles of the gas were admitted, after passage over phosphorus pentoxide. Aluminum electrodes were used, and at the temperatures produced by the current these absorb the residual nitrogen or hydrocarbons, and the spectra of argon and helium only remain. In this way he examined fifty-one minerals, and found helium in sixteen. Lockyer examined sixty-five, and found helium in fifteen; his method is, however, a little less accurate than that of Ramsay.

The fact is important that all minerals furnishing more than a trace of helium contain uranium, yttrium, or thorium, or a mixture of the three. Malacon, which yields argon and helium (the former in excess), is a hydrated silicate of zirconium. It is the only mineral which gives argon, in notable quantity, without containing uranium.

The amounts of helium varied greatly. A sample of uranite gave 13.5 c.c. per gram, a sample of cleveite yielded 6.1 c.c. per gram, a sample of broggerite furnished 1.8 c.c. per gram, while many yielded only a trace of the gas.

The question must, of course, at once arise: Is helium in solution, in combination, or occluded in these minerals? The evidence afforded by the experimental data at hand is conflicting. An examination of the gas which is evolved on heating a specimen of broggerite shows that it contains almost no free hydrogen, and no combination of helium and hydrogen could be detected in it. There is, moreover, no constant relation between the amounts of helium and hydrogen evolved when the minerals are treated with acid, hydrogen being absent in some cases.

On the other hand, Tilden allowed cleveite to remain for some time at 100° C. under a pressure of seven atmospheres, in contact with the gas which had been evolved from the mineral, and states that a small quantity of the gas was reabsorbed, indicating a recombination of the helium. Ramsay, however, operating at atmospheric pressure, was unable to confirm Tilden's results, and the evidence is accordingly less convincing.

Finally, there is the evidence afforded by the experiments upon fergusonite and yttrocerite. Fergusonite is a transparent mineral, which under the microscope shows no trace of crystalline structure; it is perfectly homogeneous and shows no bubbles (interstices). But on heating to 500° it becomes incandescent, and gives up most of its helium. The density falls from 5.619 to 5.375. There are other minerals which evolve heat under similar circumstances, but they do not become incandescent, and either yield no helium, or only a trace, and their densities are greater after than before the heating. By means of calorimetric measurements four fairly concordant values for the heat of incandescence have, however, been found to approximate 809 cal. per gram, and this would seem to show that helium forms a true compound with some constituent of the fergusonite, the heat evolved being that of decomposition at the moment of the liberation of the helium. These results are confirmed by Thomsen's experiments[1] with yttrocerite which also becomes incandescent upon heating, and evolves helium.

From the foregoing it is evident that the question of the state in which the helium exists in these minerals cannot be finally answered, but the positive evidence seems to slightly outweigh the negative and to point to a possible combination with some constituent of the minerals examined.

Helium has been found in one sample of meteoric iron, together with argon, but was absent from other samples examined. It was found in the gases issuing from the springs at Bath, England (forming 1.2 per cent. of the gas), and at some other points, but not by any means in all the gases examined. It has only been found in gases emanating from *hot* springs.

For some time it was a point in doubt whether helium is a constituent of the atmosphere. Kayser[2] and Friedländer,[3] working inde-

[1] *Zeitschr. phys. Chem.*, **25**, 112.

[2] *Chem. News*, **72**, 89.

[3] *Zeitschr. phys. Chem.*, **19**, 657.

pendently, claimed to find it, but Rayleigh and Ramsay were unable to detect it. When, however, large quantities of liquid air were worked over in the search for the possible companions of argon, it was soon found that helium was beyond question present in the earth's atmospheric envelope.

The preparation of helium in large quantities in a state of purity has proven to be a matter of grave difficulty, on account of the fact that the gas must be handled over mercury in order to prevent contamination by argon, which, on account of the chemical inertness of both substances, can only be separated by means of fractional diffusion or fractional distillation, both of which are extremely tedious procedures. A separation based upon the difference in solubility in water, is possible up to a certain point, but cannot be made complete.

The sources of helium are liquid air and certain minerals, the latter being probably the more available source. To obtain the helium, the minerals are heated in a hard glass tube which has been carefully exhausted by means of a Töpler pump. The gas evolved is collected over mercury, and is then passed over caustic soda to remove carbonic acid, over heated cupric oxide to remove hydrogen, over heated magnesium to remove nitrogen, and over phosphorus pentoxide to remove water. The hydrocarbons are, of course, removed at the same time with the hydrogen.

The most accurate results in the determination of the density of helium seem to place the value at 1.99. The ratio of the specific heats has been found to be 1.65, which indicates that the atomic weight of helium is 3.98.

Dewar at first supposed that he had liquefied helium at the temperature of melting hydrogen, about — 260°, but subsequent experiments failed to confirm this, and it is now considered that helium is the only gas which has not been liquefied, and that its boiling-point lies below that of hydrogen, being probably about 5° absolute, a fact which is confirmatory of its monatomic character. Repeated diffusion experiments failed to give any data which would indicate that helium is other than an elementary substance. Fractions varying slightly in density were, indeed, obtained, but they showed absolutely no spectrum lines except those of helium and argon.

Estreicher[1] finds that the solubility of helium in water decreases

[1] *Zeitschr. phys. Chem.*, 31, 176.

from 0 to 25° C., and then increases. Its solubility is very small at first, but at 30° it is about equal to that of nitrogen. He points out that the curve showing the solubility of hydrogen, the next lighter gas, also possesses a minimum point, and thinks that the same would be true of all gases if their solubilities were studied at higher temperatures. The very slight solubility of helium corresponds with its resistance to liquefication.

The rate of diffusion of helium seems to be a little greater than would correspond with its density as determined by weighing. It conducts electricity with extraordinary readiness, as is shown in the tables below, which give the distances at which a spark will pass between electrodes in different gases at a constant potential, and also the pressure at which a spark discharge changes to a luminous band.

LENGTH OF SPARK (UNDER SAME POTENTIAL).

	MILLIMETERS.
Oxygen	23.
Air	33.
Hydrogen	39.
Argon	45.5
Helium	150–300.

GAS PRESSURE AT WHICH SPARK DISCHARGE CHANGES TO BAND DISCHARGE.

	MILLIMETERS.
Air	73–74
Hydrogen	42–43
Oxygen	81
Carbonic Anhydride	92–94
Cyanogen	23
Nitrogen	33
Carbonic Oxide	49
Helium	1270

Ramsay calls attention to the fact that the low index of refraction of helium (0.1245), the high rate of diffusion and electrical conductivity, all point to small molecules occupying very little space, and moving with great freedom and rapidity.

Helium appears to be as inert as argon under the conditions of experimentation. In the attempt to study the chemical activity of helium, the gas was passed through a hard glass tube containing the substance which was to be tested, and the gas remained in contact with it for at least one-half hour, under such conditions that a change in volume of 2 or 3 c.c. could easily have been detected. The tube was afterward cooled, and the gas removed by the pump. The results were negative in every instance. The substances tested were similar to those enumerated under argon.

Helium is apparently slowly absorbed by platinum when the platinum is spattered on the walls of the vacuum tube; but this is true only to a small extent, and the nature of the absorbent action is still obscure. Ramsay suggests that Berthelot's results with benzene in the case of argon and helium may be of a similar character.

The spectrum of helium is exceptionally beautiful, showing lines in the red, yellow, green, and blue of marked brilliancy. Lockyer was inclined to believe that the spectrum attributed to helium was really that of more than one element, basing his belief upon the fact that the spectra given by the fixed stars varied somewhat, and did not always exhibit all the important lines. Ramsay, in reply to this statement, expresses his conviction that variations of distance, temperature, and other conditions may very well occasion the observed fluctuation in the spectra, and does not consider that this evidence can outweigh the fact that no differences of density can be detected by careful diffusion experiments with helium.

A contention of Runge and Paschen[1] that helium comprised at least two elements deserves consideration on account of the general acceptance which was for a time accorded it. These observers found that the lines of the spectrum of helium could be divided into two sets, and that each set consisted of a primary and two secondary series of lines, of which the vibration frequencies could be expressed by a logarithmic formula. This they attributed to the presence of two gases, and gave to the lighter gas — that is, to the one with its spec-

[1] *Math. nat. Mitt.*, (Berlin) 1895, 323.

trum nearer the violet end — the name " par-helium," and to the other "helium." The former exhibited a spectrum in which the green line was very pronounced, while the latter gave the characteristic yellow line. Their observations seemed to be confirmed by the fact that if helium was allowed to partially diffuse through a porous plug, the gas which diffused showed a green glow, and the bright green line.

This fact that the helium spectrum contains two sets of lines of which one comes into prominence at one time, and the other predominates at another time has not been explained but it has been shown that mere change of pressure in the vacuum tube will occasion this variation, and it has also been shown that similar conditions exist in connection with oxygen, and if these facts are to serve as arguments for the dual character of the gas in one case, they must in the other.

Additional evidence of the simple character of helium is found in the fact that helium is partially absorbed by the platinum spattered on the sides of the vacuum tubes, and as this absorption progresses, the glow changes from yellow to green. If this indicated the removal of one constituent of a mixture the spectrum of the gas liberated from the platinum should show the yellow line without the green line, or the latter should at least be weak. In fact, the liberated gas shows the same behavior as the original gas from cleveite.

The properties of helium are stated in the table on page 227. It is also worthy of note that helium shows little variation from the gas laws, and is therefore of value for use in thermometers, especially at low temperatures. It has been used to measure the boiling and freezing point of hydrogen.

Discovery of Other Members of the Group.

In December, 1898, Professor Ramsay, in a lecture before the *Deutsche Chemische Gesellschaft*, gave an account of the steps which led to the discovery of the other members of this unusual group. The fact has already been mentioned in connection with both argon and helium that their molecules appear to be monatomic, if we accept as conclusive the evidence furnished by the ratios of their specific heats at constant pressure and constant volume. Some doubts to be mentioned later have been raised as to the validity of this proof because of the conflicting arguments to be brought forward from the point of view of the periodic system. If argon has an atomic weight 40, then, as we shall later see, it does not fit into that system, since this would place it

between potassium and calcium. On the other hand, the assumption that the atomic weight of helium is 2 instead of 4, while less troublesome, is still open to objection. Chemical evidence by which to determine these points is, of course, lacking. Ramsay states that while he preferred to accept the physical proof of the monatomic character of the two elements, leaving their exact relations to the rest of the periodic system for later settlement, he also determined to attack the problem from a standpoint based upon the general relationship of these elements to that system.

He pointed out that if the atomic weights were assumed to be 2 and 20 there would be no place for an element of this group between argon and helium, since each pair of the elements in the various groups in this portion of the system shows a uniform difference of about 16 or 18 in atomic weights. On the other hand, if the atomic weights 4 and 40 are assumed to represent the truth, then there is a space to be filled by an element with an atomic weight about 16 higher than helium, and 20 less than argon. He pointed out that the discovery of such an element would not only be confirmatory of the atomic weight 40 for argon, but also of the significance of the specific heats of gases as a criterion of their molecular weights. He accordingly began at once a search for such an element.

This quest, begun by Ramsay and Collie and continued by Ramsay and Travers, opened with the careful examination of the same minerals which had proved to be a source of helium, as well as a considerable number of other minerals of different chemical character. The results were, however, entirely negative. Helium was found in a number of minerals, in one or two argon was present in small amounts, but in no case could lines be detected by the spectroscope which were not characteristic of known elements. Meteorites next received attention, but of seven of these, one only gave a little helium and a trace of argon. The gases from mineral springs were alike unproductive.

At this stage the search appeared to be in a high degree discouraging. One hope remained however. The atomic weight of argon would be more tractable if it could be reduced below 40, and a search after a constituent of the atmosphere having a lesser density than 20 seemed worth the making, in view of these considerations: first, that Dr. Johnstone Stoney had made a statement, based upon calculations, that while an element as light as helium could not probably be retained in the earth's atmosphere, an element having a density of 9 or greater,

would probably be so held; and second, because in the diffusion experiments in the purification of argon a single portion of gas of lesser density had been obtained, but, as at that time helium and its sources seemed a more promising field, this clue had not been followed up.

It was next determined to work upon large quantities of argon, studying it in the liquid condition. Some months were required for its preparation, special tubes, furnaces and gas holders being necessary.

The oxygen was removed from the air by means of phosphorus, until about 200 liters of atmospheric nitrogen were collected. This was dried by sulphuric acid and phosphoric anhydride. Then the gas was repeatedly passed over magnesium at a red heat, to remove nitrogen; then over copper to remove hydrogen, the water being again absorbed as before. For the final removal of nitrogen, the gas was passed over a mixture of lime and magnesium at a red heat; then over copper oxide again, to remove hydrogen, as the lime could not be entirely freed from water. In connection with this procedure, the question whether the gas sought for could possibly remain in combination with the magnesium was investigated. The magnesium nitride was decomposed, but only ammonia, hydrogen, and a trace of hydrocarbons were found. The nitrogen from the ammonia was found to be identical with nitrogen from nitrites, etc. The examination of the residual magnesia (after treatment of the nitride) was not completed, as other lines of work developed positive results at that time.

In preparation for the liquefaction of the argon, Ramsay and Travers made some preliminary experiments with liquid air simply to acquire facility in manipulation. In the course of these, Ramsay remarked that it seemed a pity to have the liquid air boil away without examining the last fraction as to its character. This residue was collected, and after removal of the oxygen — of which, with argon, it was chiefly composed — and of the nitrogen, it was found that there were present new spectrum lines, notably a yellow line not identical with that of helium, and a new green line. The gas had a density 22.5 instead of 20, and again the ratio of specific heats of the mixture of gases was found to closely approximate 1.66. Here then was a new element, or elements, but not the one sought for. Ramsay, assuming the presence of one only, gave the name "krypton" (concealed) to it; of its properties more will presently be said.

Ramsay then proceeded to liquefy the argon which he had purified. It yielded 25 c.c. of a clear colorless liquid, containing flocks of a

white solid. By increasing the pressure on the liquid air surrounding the argon (it was first reduced to 10-15 mm.) the temperature slowly rose, and the argon began to boil. A first fraction was collected separately, then the major portion of the gas was allowed to return to the gas holder; a final portion was again collected separately. A solid substance remained with some persistence, but gradually evaporated when the vessel was connected with a Töpler pump.

The light fraction was first examined. Its density was 14.67; the ratio of specific heats, 1.66; the spectrum showed, beside the lines of argon, a number of new lines of red, orange, and yellow of marked brilliancy. Here again was a new element or elements. The process of liquefaction was again applied to this gas, but this time not all could be made to liquefy at temperatures obtainable by liquid air, even after repeated attempts with the gas under an atmosphere's pressure. This non-condensible gas was separated and again examined. The density, to the great satisfaction of the investigators, was found to be 9.76. Some argon was, however, still present. The new gas, which was designated as "neon," would then, when pure, probably have a density lower than 9.76. But later spectroscopic examination showed the presence of helium, which, on the contrary, would decrease the density of the mixture.

The next problem was how to remove these elements, argon and helium. How this problem was solved will be explained in a moment. It suffices here to say that neon proved to be the missing member of the helium-neon-argon triad for which Ramsay was seeking.

In the address already referred to, Ramsay stated that they had not had time to investigate the other components of the less volatile portions of the "argon," but they considered that it contained at least three gases, — two beside krypton and argon. One of these they named "xenon" (stranger), but at that time they obtained it only in an impure condition, studying its spectrum, and determining its density as 40.5 to 41.1. Of this also more will be said later.

The fourth component of the less volatile portion was given the name "metargon." It was at that time a source of much perplexity to the investigators, and its spectrum was found to resemble the so-called "swan spectrum" of carbon monoxide. Subsequent study showed that the supposed new element was in reality carbon monoxide, which had been, as it proved, introduced by the presence of carbon in the phosphorus employed to remove the oxygen from the original air. This

was not suspected by the investigators, and caused no little annoyance and labor. It is a striking example of the difficulties which beset investigators, and of how the most careful observers may be temporarily led astray. "Metargon" must, then, be dropped from the list of the newly identified elements.

To determine how far the density of argon was influenced by the presence of these other gases, 25 c.c. of fluid argon was fractionated three times — 200 c.c. of the most volatile and 200 c.c. of the least volatile portions of the gas being separated each time. Then the middle portion was liquefied, and on evaporation, six fractions were collected and examined. The densities are given in the table:

	Density.	Refractivity.
1.	19 65	0.962
2.	19 95	0.969
3.	19 95	—
4.	19.91 (subsequently 19.96)	—
5.	19.97	0.968
6.	19.95	0.966

Of the newer elements only neon is present in sufficient amount to appreciably influence the density of argon, and then only in the second decimal. Xenon could not be detected at all in 15 liters of argon gas.

The latest results place the amounts of these elements in the atmosphere at approximately the following figures:

Helium	1 to 2	parts per million
Neon	1 to 2	" " hundred thousand
Argon	0.937	" " hundred
Krypton	1	" " million
Xenon	1/20	" " million

It seems probable, according to Stoney, that helium continually passes out of the atmosphere, owing to its lightness, but in the aggregate the amount issuing from mineral springs is enormous, and maintains the proportion in the atmosphere at a constant quantity.

The methods employed to separate helium, neon, krypton, and

xenon from argon and from each other deserve brief consideration. A detailed description of the fractionation processes is to be found in the recent volume by Travers, entitled, "The Experimental Study of Gases." Only an outline of the essential features will be given here.

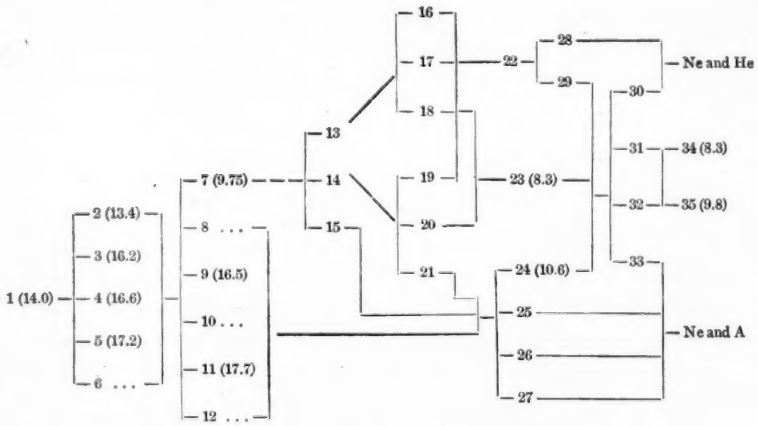
The spectroscope was first used to test the purity of the gases and to note the progress of the separation, but it was found that this instrument would not show traces of one gas in another, as is evident from the table given below based upon a statement made by Ramsay. As an ultimate test of the impurity or purity of the gases it was therefore found necessary to resort to repeated determinations of density, with intermediate diffusion or fractionation. If successive weighings under these conditions gave constant results, the gas was considered to be pure. It was found possible to obtain accurate determinations of density by using a globe of 7 c.c. capacity if the weighings were completed to one one-hundredth of a milligram.

PER CENT BY VOLUME OF GAS CAPABLE OF DETECTION BY MEANS OF THE SPECTROSCOPE IN MIXTURES:

GASES.	PER CENT.
Helium in Hydrogen	10.
Hydrogen in Helium	.001
Nitrogen in Helium	.01
Helium in Nitrogen.	10.
Argon in Helium	.06
Helium in Argon	Over 25 at 0.58 mm. pressure
Nitrogen in Argon	" .08 at 0.18 mm. pressure
Argon in Nitrogen	37.
Argon in Oxygen	2.3

The process of fractionation was as follows: The gas was passed from the gas-holder into an apparatus provided with a small bulb surrounded by liquid air, the air being made to boil at a pressure of a few millimeters. At this temperature the gas condensed to a liquid. By increasing the pressure upon the liquid air its boiling-point was raised, and the liquid within the bulb distilled slowly. The gases thus liberated were allowed to pass into gas-holders, these holders being

changed just as the receivers are changed during the fractional distillation of a mixture which is liquid at ordinary temperatures. The gas was pumped out of the bulb into the gas-holder at the end of the operation, or at other times, when necessary. After the liquid had completely evaporated and had thus been divided into fractions, these fractions were recondensed in turn and allowed to evaporate and were thus refractionated. In this process the less volatile portion of the first fraction introduced is recondensed with the second fraction, and so on. For the separation of argon, neon and helium, the *heaviest* fraction was first recondensed, and to the residue from this the next lighter was added, and so on, while to separate the gases which are heavier than argon, the *lightest* fraction was first recondensed, then the next heavier, and so on. The scheme of fractionation given below illustrates the procedure in the attempt to isolate neon. The numbers in parentheses represent densities.



As it was found that fraction No. 7 could not be condensed even under pressure at a temperature obtainable with liquid air, it was necessary to mix the gas with a considerable quantity of oxygen and to continue the fractionation of this mixture. The oxygen was removed before determining the density of the fractions. In this way the separation was carried to the point indicated in the scheme, but the helium could not be separated from the neon, and the volume of the mixture of the two had been reduced so low that further work was impossible until a larger quantity of material was available. In the preparation of this,

Ramsay and Travers made use of the fractionation principle in the initial preparation of liquid air. A considerable portion of air was liquefied, and then a small portion of this was allowed to distil, which was then transferred to the compression apparatus and re-liquefied with new portions of air. By repeating this procedure, they ultimately obtained about 120 liters of air which was distinctly lighter than common air, and from this, after removal of oxygen and nitrogen, about 800 c.c. of argon which was comparatively rich in neon.

It is interesting to note that it is necessary to agitate the liquid air during distillation in order to obtain the neon. Unless this is done the evaporation takes place from the surface of the liquid only, and the first product is mostly nitrogen and argon. This is said to be due to the fact that the nitrogen becomes superheated.

With this new quantity of argon, neon, and helium, the fractionation processes were repeated, as already described, and nitrogen was substituted for oxygen, but without improvement. A point was reached beyond which the separation could not be carried by this means. The densities varied from 7.24 to 8.27.

Diffusion methods were next tried, but were only partially successful, although the density increased to 9.18, with no evidence of argon.

"The separation of these two gases was ultimately accomplished by liquefying them in a bulb cooled by liquid hydrogen. At this temperature the neon condensed, possibly to a solid, and its vapor pressure was not more than 50 mm.; the helium appeared to remain gaseous, while the argon was non-volatile. After condensing the gas the first fraction was taken into the pump; it contained a large quantity of helium. The second fraction, which evaporated slowly, was collected separately, and after removing the vacuum-vessel containing the hydrogen, a third fraction containing argon was collected. The second fraction was then recondensed, a small quantity of it was allowed to evaporate into the pump, and the main bulk of the gas was collected as pure neon; the last traces of the gas were again discarded, since they might contain traces of argon.

The fraction of the gas supposed to be pure neon was found to have the density 10."

The separation of krypton and neon from argon and from each other, was only effected by a long series of fractionations, involving the utmost patience and a high degree of manipulative skill. A large quantity of liquid air was allowed to evaporate quietly, and the less

volatile portions were collected and freed from oxygen and nitrogen as already mentioned. The residual gas had a density 22.5 — a value somewhat greater than that of argon. The investigators were surprised to find that the density of the fraction which was supposed to contain the heavier krypton was less than that of the initial gas, although it showed the krypton lines. An explanation of this was subsequently found in the low vapor pressure of xenon at the temperature of liquid air which caused it to be held back in the bulb. This property of xenon was ultimately utilized to separate it from krypton.

The scheme of fractionation is similar to that noted for the separation of the lighter gases. After the argon had been practically separated, it was found that xenon solidified at the temperature of liquid air, while krypton had a vapor pressure of about 150 mm. It was therefore necessary to remove the krypton from the fractionation bulb by means of a pump. The separation of xenon and krypton, (which was more tedious than difficult) was retarded by the fact that the latter seemed to be enclosed in, or to be soluble in, the solid xenon. It was finally possible for Ramsay and Travers to isolate about 16 c.c. of a gas which withstood their tests for purity, and which they regarded as pure krypton. The amount of xenon isolated was about 3 c.c. The former gas had a density 40.75; the latter, 64. In both cases, the ratio between the specific heats is approximately 1.66, indicating molecular weights of 81.5 and 128, respectively. The remaining properties are stated in the table on page 227. The spectrum of krypton is easily detected in the presence of other gases; the spectra of krypton and xenon undergo a corresponding change to that of argon when the character of the electric discharge changes.

So far as is now known, all of these elements are as inert chemically as argon.

Since the completion of the work of Ramsay and Travers, Ladenburg and Krügel[1] have prepared a fresh quantity of krypton, starting with 850 liters of liquid air. From this they obtained about 3 liters of the less volatile residues, which gave about 3.5 liters of gas after the removal of the oxygen and nitrogen. This was condensed, the argon removed by fractionation, and finally a crystalline solid was obtained, from which by fractional evaporation they obtained a gas which appeared to be of constant composition, showed the krypton

[1] *Kgl. Akad. Wiss. Berlin* (1900), **16**, 212 and 727.

lines with brilliancy, and after repeated fractionation the fractions showed densities of 29.33, 29.50 and 29.40, from which they conclude that the atomic weight of krypton is about 59 instead of 80, as determined by Ramsay. Although this work was published in 1900, Ramsay appears to have made no comment upon it and Travers does not mention it in his book recently issued. Its bearing upon the question of the position of these elements in the periodic system will be discussed presently.

Liveing and Dewar[1] have recently made an interesting study of the most volatile and the least volatile gases of the atmosphere which merits a moment's attention. They collected about 200 c.c. of liquid air from the roof of a building in London, cooled it to —200° C., and then allowed the more volatile portions to distil into a vessel cooled to —210° by liquid hydrogen. The liquid then condensed to a solid. When the equivalent of 10 c.c. of liquid had solidified, it was allowed to evaporate and passed into vacuum tubes through a tube surrounded by liquid hydrogen, which condensed all but the most volatile gases. The spectrum showed helium, neon, and hydrogen, and some new lines which may belong to these elements, or may represent bodies as yet unidentified; on this point they withold a positive statement. They searched for lines corresponding to those found in the spectrum of the corona, aurora, and nebulas, but while they found close approximations to these lines, they could not state positively that the correspondence was complete.

A most interesting outcome of this work was the discovery that hydrogen is present in the atmosphere. When the tube surrounded by liquid hydrogen between the distilling gas and the vacuum tube was omitted, the gas was found to contain very considerable quantities of hydrogen. In view of Dr. Johnstone Stoney's statements that helium cannot be retained in the atmosphere of the earth, this discovery raises a number of interesting questions, — among others, what is the source of this hydrogen?

For the examination of the *least* volatile gases, the residual portions of the liquid air were condensed to a solid by the aid of liquid hydrogen, and it was found that by slowly raising the temperature of the solid the components gradually evaporated, and that the change from one to the other was surprisingly sharp. By means of a sparking tube

[1] *Proc. Roy. Soc.*, **67**, 467 and **68**, 392.

through which these gases passed, the progress of the separation could be accurately followed. They found that the spectra of both krypton and xenon change with the character of the discharge and they observed unaccountable variations in the spectra of these elements, such as the appearance, on a single occasion, of unknown lines in the xenon spectrum which may indicate the presence of another body, but of this, again, they are as yet unwilling to speak with certainty.

Relations to the Periodic System.

There remains only the discussion of the relation of the members of this group to the periodic system.

A very considerable number of papers bearing upon this question are to be found in the journals; but, as many of these were written before the discovery of helium, and most of them before the knowledge of the existence of neon, krypton, and xenon, the speculations of many of the authors need not be reproduced here. It is worthy of mention, however, that the existence of elements with atomic weights approximating those of the members of the argon group, and probably without chemical activity, had been predicted by Flawitzsky, Julius Thomsen, and Boisbaudran. Flawitzsky and Thomsen arrived independently at the same conclusions, about 1887. They noticed that while the electro-chemical character of the members of a specific series change gradually, the change from the negative element of one series to the electro-positive element at the beginning of the next is most abrupt, and within a few units difference in atomic weights. If the properties of the elements are periodic functions of the atomic weights, then mathematical considerations would lead to the conclusion that such a change from negative to positive must theoretically involve an intermediate stage corresponding to zero or infinity — the latter case according with a sudden change, such as that noted. Thomsen calculates values for atomic weights of elements the electrochemical character of which should be $\pm\infty$ (that is, of no valence) as 4, 20, 36, 84, 132, 272, 292, — values which in part accord fairly with the atomic weights of the argon group, although he was disposed to assign to argon an atomic weight 20.

On the basis of a study of the forms of the atoms, Sedgwick arrived, in 1890, at the conclusion that there should be an inactive element possessing an atomic weight about that of argon, the atom of which should be spherical in form; and, since according to his

notions, the valence of an element was the consequence of the existence of flattened surfaces on the otherwise spherical surfaces of its atoms, such elements should show no tendency to unite with others. Subsequently he also predicted an inactive element of the atomic weight of neon.

Boisbaudran, shortly before the discovery of argon, arranged a classification of the elements with reference to their atomic weights, from the study of which he predicted the existence of elements with atomic weights 20, 36.5, 84, and 132, of non-metallic character and even atomicity. He was inclined to regard argon as the first representative of this series. No comment has apparently been made by Boisbaudran regarding the other members of the argon group.

Perhaps the first question to be asked is: Does our present knowledge of these recently discovered substances justify us in attempting to assign them places in Mendelejeff's system? Have they been shown to be elements in the sense in which the other members of the system are so regarded? These questions have been variously answered.

Rayleigh, Ramsay, and Travers are plainly inclined to regard these bodies as elementary in their character. That is, they do *not* regard them as made up of any combinations of atoms of known or unknown elements, and they *do* regard the gases which they finally weighed, and to which they assigned atomic weights, as essentially pure and free from other as yet unidentified substances, and they further regard the members of the argon group as entitled to a place in the periodic system, to be determined as far as possible by the *physical* properties which they exhibit.

On the other hand, Piccini[1] maintains that Mendelejeff's classification is based essentially on chemical principles; that the physical properties of the elements are subordinate with respect to this classification, and that in the absence of any chemical data, these bodies cannot be regarded as "elements," which Mendelejeff defines as "those material constituents of simple or compound bodies which occasion their physical and chemical behavior." Piccini appears, however, to be a rather ardent advocate of the infallibility of the Mendelejeff classification as it stands, and seems to fear that an attempt to force these new members into some place in the system will tend to discredit the whole principle. It is probable that his contention that argon and

[1] *Zeitschr. anorg. chem.*, 19, 295.

its associates have no proper place in the periodic system will hardly find general acceptance.

To return for a moment to the question whether these members of the argon group are to be regarded as elements, a very recent paper by Martin[1] should be mentioned, in which he contends that their elementary character is not proven beyond question, and cannot be, in the absence of chemical reactions. He remarks that argon might, for example, be a mixture of gases varying but little (one or two units) from each other in atomic weights, — as do iron, cobalt, and nickel. To this no positive reply can be made; yet, in the case of argon, it would appear that in such a long series of fractionation experiments some differences in density or spectrum would point to the presence of a mixture of gases, if such existed. The experiments of Olzewski with the liquid gas, which indicate a definite and constant boiling and freezing point, confirm this position.

Assuming then, for the moment, the elementary character of helium, argon, etc., what atomic weight shall be assigned to them? The densities as compared with hydrogen (1.008), as given in the table (page 227), are experimental data, free from all hypothesis, and the molecular weights are twice these values.

But what shall we say as to the number of atoms in the molecules? Shall we accept the deductions from the ratios of the values for the specific heat at constant pressure and constant volume, and say that the molecules are all monatomic? Again there are dissenting voices.

It is pointed out that that argument depends upon an assumed actual collision of the molecules, whereby a part of the energy of translatory motion is converted into rotational motion of the parts of the molecule, and that if, because of a repulsive force between the molecules, such actual contact does not occur, the basis for the deduction may not, at least in the case of molecules with compact structure, be sound. It is further noted that there may be molecules whose atoms are so closely united that the molecule is practically spherical in form, and in which rotational energy cannot, therefore, be detected; again, that the ratio of the specific heats in the case of avowedly diatomic substances varies considerably, and seems in some cases to grow less as the chemical activity increases, as in the case of chlorine, which if the reverse were true, might explain the high value for these elements, (this is not, however, borne out in the case of oxygen and nitrogen); once

[1] *Proc. Chem. Soc.* (Lond.), **17**, 259.

more it is noted that a monatomic element resembles in some degree a nascent element, and this seems incompatible with chemical inertness; and finally, that argon, if considered alone, would fit rather better in the system with an atomic weight 20 than one of 40. Most of these arguments appear to antedate the discovery of the newer members of the argon group, and taken in connection with the behavior of mercury, they do not appear sufficiently convincing to overthrow the previous assumption of monatomicity, which finds support in the fact that argon and helium, at least, diffuse more rapidly than the polyatomic gases, and in the very low boiling-point of argon, and especially of helium, which, if its molecule were diatomic, should be higher than that of hydrogen.

Without attempting to settle the question finally, let us assume momentarily the validity of the proof of the monatomic character of the atoms of all of these elements, and see to what it leads. A brief examination of the periodic system suffices to show that argon with an atomic weight of 40 cannot be made to fit smoothly into the scheme. With this exception, the elements will find a place in the eighth group if we assign to them the values for their atomic weights determined by Ramsay. He inserts them between the halogen group and the sodium group, as shown by the table below. It is to be noted here that hydrogen becomes the first member of the halogen group — a position for it which has been defended by Professor Masson.[1]

H	He	Li	Be
1	4	7	9
F	Ne	Na	Mg
19	20	23	24
Cl	A	K	Ca
35.5	40	39	40
Br	Kr	Rb	Sr
80	82	85	87
I	X	Cs	Ba
127	128	133	137

[1] *Chem. News*, 73, 280.

This does not, however, account for the anomally in the value for argon, which should place that element on the other side of potassium, but Ramsay remarks that the difficulty here is no greater than with tellurium, or cobalt, both members of the eighth group at present.

It may be remarked here that Ramsay regards it as improbable that an element of the argon group heavier than xenon will be found, arguing in part from the analogies between xenon and iodine, and caesium and barium. His experiments indicate that such an element can only exist in the atmosphere in extremely minute quantities, if at all.

To show how far some of the observed properties of these elements agree with the requirements of the periodic law, the curve (Fig. 1) representing the refraction equivalents of the various elements, and the curve

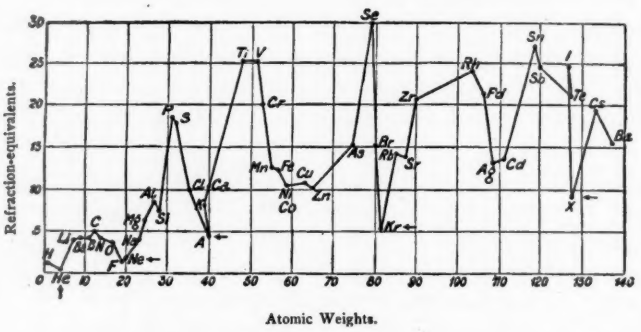


FIG. 1.—RELATION BETWEEN REFRACTION-EQUIVALENTS AND ATOMIC WEIGHTS. (From Ramsay and Travers.)

(Fig. 2) showing their atomic volumes, are reproduced here. It will be noted that, in the one case, the elements of the argon group are all at minimum points of the curve; in the other, they occupy corresponding positions on the ascending portions of the curve, except that the position of argon is abnormal in each case. A curve showing the melting-points exhibits the same approximation to the law as in these two instances. The similarity in position of argon and tellurium should be noted.

It is evident that the value for the density of krypton, as determined by Ladenburg and Krügel (from which the atomic weight would be about 59) would throw this element entirely out of line with the others. Of the merit of the work of these investigators, as compared with that of Ramsay and Travers, it seems impossible to express a

well-founded opinion, and one can hardly do more than merely to present the result of their work, without attempting to draw conclusions from it.

James Lewis Howe[1] has given some reasons for the assignment of the helium-xenon family to the eighth group, which deserve some mention. He points out, as already noted, that the position assigned to argon, if we assume its atomic weight to be 40, is no more anomalous than that of cobalt and tellurium. He suggests that it is not impossible that atomic weight of an element need not in every case have a value which is intermediate between those of the elements between which it

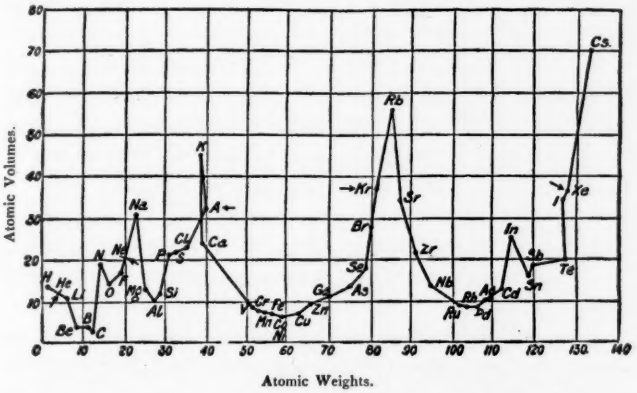


FIG. 2. — RELATION BETWEEN ATOMIC VOLUMES AND ATOMIC WEIGHTS. (From Ramsay and Travers.)

lies with respect to its properties; that is, he conceives it to be possible that an element may occasionally have an atomic weight slightly greater than the element which follows it, without necessarily overthrowing the system. He suggests that the atomic weight may be made up of two factors; for example: αn and βm. αn is relatively large and simple, and βm small and complex. In most cases, the atomic weight increases as αn increases, but with certain elements the quantity βm may have sufficient influence to cause the atomic weight to temporarily retrograde. He points out that if we neglect the abnormal value in the case of argon, and place the members of this group of inactive elements in group eight of the periodic system, arranged according to the suggestion of Venable, that these elements

[1] *Chem. News*, 80, 74.

form a transition group from the halogens to the alkalis, except that there is no halogen of lower weight than fluorine. He points out that the valence toward hydrogen is one in the seventh group, and would naturally be zero in the eighth; that the valence toward chlorine and other similar elements in the first group is one, and would therefore be preceded by a valence of zero; that the valence toward oxygen is affected by the electro-chemical character of the elements, so that, since fluorine forms no oxide, we should expect no oxygen compounds of this member of the eighth group.

Venable gives to the members of the first period the name of "group" elements; to those of the second period, "type" elements, since it is they, rather than the group elements, which foreshadow the character of the succeeding elements of the respective groups. In this classification argon would appear as a type element, and the question at once arises: Can it be associated in this respect with any other elements of the eighth group? If we examine Venable's table, we find that, while in group I, the elements of the positive series closely resemble the type element argon, in group II, the positive series resemble magnesium less closely, and when we pass to group VII, the only element of the positive series, manganese, bears very little resemblance to the type element fluorine. It is not surprising then, that in group VIII this resemblance entirely disappears, and we find no similarity between iron, cobalt, and nickel on the one hand, and argon on the other. A similar examination of the table, with respect to the negative series, would lead to the same conclusion which Howe advances as a reason for assigning these new elements to that part of the system.

He considers the fact that helium, neon, and argon form a triad as strong evidence in favor of the elementary character of these type elements.

It is also true that the more recent work on the elements of the argon group with higher atomic weights, viz: krypton and xenon, have assigned to them atomic weights which will place them in the same relations to bromine and iodine that argon has to chlorine, if we place that element in the eighth group, neglecting, however, the value 59 for krypton as already noted.

Finally, it should be noted that Crookes has arranged the elements upon a series of figure-eight curves, which combine the spiral and zig-zag, and he arrives at the same conclusions as those just stated,

that the argon family finds a place in the eighth group, as transitional between the seventh and first groups. He regards this absence of chemical properties as the result of the pause in the swing of the pendulum between the electro-negative and the electro-positive elements. He, too, would place hydrogen as the first member of the halogen series.

The views of Howe and Crookes are representative with respect to the position to be assigned to the members of the argon family in the Mendelejeff system. Exception is taken only by those who hold that these substances are types of a new class of bodies for which we have no precedents or place, and who would, therefore, exclude them, at least temporarily, from the system altogether, to await future discoveries. No explanation, other than that of Howe, is offered for the anomaly in the case of argon; nor can we explain the apparent exceptions in the cases of tellurium and cobalt. Until we gain new light as to the meaning of the approximate periodicity of properties observed in most of the elements, or until our observations of these properties or our determinations of atomic weights have been proven faulty, perhaps we can do no better than to assign these elements (assuming them to be such) to the somewhat motley collection in the eighth group.

Such, then, is the outline of the recorded history of the members of this unique group, in which, as their discovery is a product of the work of our own time, we may properly take a deep interest. If I tell you that there are already over 250 articles in the journals upon these gases, it will be plain to you that what has been said represents but a portion of the material to be drawn upon. I can only hope that it includes the larger and the more important portion.

The following papers contain reviews of the published work to the dates named: (1895) Rayleigh and Ramsay: *Chem. News*, 71, 51; (1896) Ramsay: "The Gases of the Atmosphere," Macmillan; (1897) Parker: *J. Am. Chem. Soc.*, 19, 134; Mugdan: "Argon und Helium" in the "Sammlung chem.-techn. Vorträge"; (1898) Ramsay: *Ann. chim. phys.*, [7] 13, 433; Ramsay: *Ber.*, 31, 3111; (1900) Ramsay and Travers: *Ztschr. phys. Chem.*, 38, 641; (1901) Travers: "The Experimental Study of Gases," Macmillan.

The principal physical properties of the members of the argon group are stated in the appended table:

PHYSICAL PROPERTIES.

	Helium.	Neon.	Argon.	Krypton.	Xenon.	[Hydrogen.]
Density. H = 1.008	1.98	9.96	19.96	40.78 (39.40)	64.	
Atomic Weights.	3.96	19.92	39.92	81.56 (58.80)	128.	
Refractivity. Air = 1.	0.124	0.235	0.968	1.450	2.368	
Melting-point.	?	?	−187.9°	−169°	−140°	−257°
Boiling-point at 760 mm.	(−268°?)	about (−243−233°)	−186.1°	−151.67°	−109.1°	−252°
Critical Temperature.	?	below (−210°)	−117.4°	−62.5°	−14.75	−240°
Critical Pressure.	?	?	40,200 mm.	41,240 mm.	43,500 mm.	11,600 mm.
Density of Liquid.	0.3?	1.0?	1.212	2.155	3.52	
Atomic Volume.	?	?	32.9	37.8	36.4	

One liter liquid air yields $\frac{1}{10}$ — $\frac{8}{10}$ c.c. xenon.
Lowest temperature obtained with liquid hydrogen is about −260°.

AN APPARATUS FOR COLLECTING SAMPLES OF WATER AT VARIOUS DEPTHS.

By EARLE B. PHELPS, S.B.

Received December 23, 1901.

For many years chemists engaged in the sanitary analysis of water have recognized the value of the oxygen dissolved determination as a rough indication of the condition of a ground water. Drown[1] has given us some valuable information regarding the relation of the dissolved oxygen in ponds and reservoirs to the depth. Whipple and Parker[2] have shown the effect of the oxygen and the carbon dioxide in a water upon the organisms which it contains.

The methods usually employed for securing samples of water for a chemical analysis cannot be used for an oxygen dissolved sample owing to the rapid absorption of the atmospheric oxygen by the water. The method generally employed for taking a sample for oxygen dissolved consists in dropping a rubber hose into the water and, by means of a suction pump, forcing a stream of water into the bottom of the sample bottle. This process is continued until water amounting in volume to several times the capacity of the bottle has passed through it, thus washing out all water which has come into contact with the air. A second method, often used, consists in fitting a sampling bottle with two tubes. One, the inlet tube, extends to the bottom of the bottle and is carried through the stopper and about an inch above it. The other tube has its lower end just within the stopper and is connected with the open air by a rubber tube. Since there is no overflow possible, the sample has been in contact with the air in the bottle for a short time.

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[1] On the amount of Dissolved Oxygen contained in Waters of Ponds and Reservoirs at Different Depths, Report of the Massachusetts State Board of Health, 1891 and 1892.

[2] Whipple and Parker: Dissolved Oxygen and Carbonic Acid in Water and their effect on the Micro-organisms, Trans. of the Amer. Mic. Soc., Vol. XXII.

Being under the necessity of taking a large number of samples for the determination of dissolved oxygen, in a recent study of the Springfield Water Supply by the State Board of Health, the writer has devised an apparatus for this purpose which, he believes, combines the accuracy of the cumbersome pump and hose method with the simplicity of the second, or bottle, method described above. The device is so simple that it has, no doubt, been previously used; but so far as the writer is aware, it has never before been described.

The arrangement of the sampling bottles is shown in the accom-

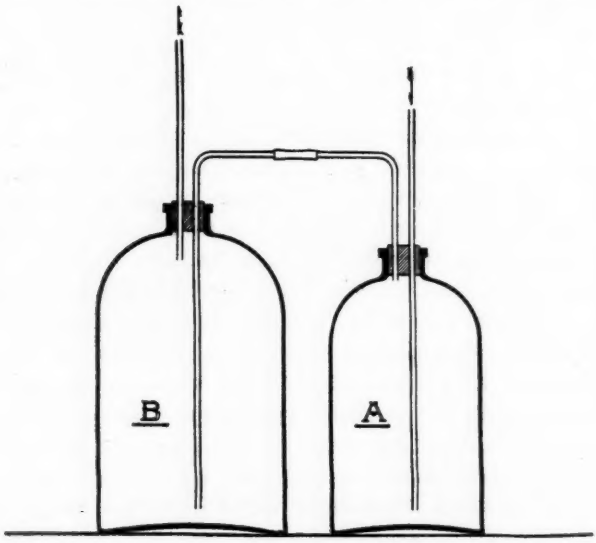

FIG. 1.—APPARATUS FOR COLLECTING SAMPLES OF WATER AT VARIOUS DEPTHS.

panying diagram. *A* and *B* are two bottles of about 250 and 500 c.c. capacity respectively, and are fitted with glass and rubber connections as indicated. They are enclosed in a suitable weighted wire cage to which is attached a suspending cord with graduations indicating the depth.

The method of operating is as follows: The cage containing the bottles is dropped quickly to the desired depth. The tube of the bottle *B* being higher than that of the bottle *A*, water will flow in through *A* and air pass out through *B*. *A* will be filled with water which will then overflow into *B*. When *B* is also full, which condition is shown

by the cessation of air bubbles rising to the surface, *A* has been filled three times. The water which entered during the descent has all been washed over into *B*, and the water which *A* now contains has not been in contact with the air. Thus one obtains a fair sample of the water at the particular depth at which the determination is to be made. The cage is hauled to the surface, the rubber stopper removed, and a glass stopper quickly inserted. The reagents are then added as usual.

The temperature may be determined with sufficient accuracy in the bottle *B* after bringing it to the surface. A very convenient method for a more careful study of the temperature is to use in connection with this apparatus a Whipple Thermaphone.

Although devised primarily for the collection of samples for the oxygen dissolved determination, this apparatus may be used for a variety of purposes. The writer has frequently used the water in the bottle *B* for the determinations of the micro-organisms.

It may appear that the bottle *B* is larger than an accurate determination requires. An experiment to determine this point was made as follows: A few drops of a strong solution of Fuchsine were added to the bottle *A* and a stream of water allowed to flow through the apparatus. By this means the actual course of the water through the apparatus could be readily followed. It was found that many eddy currents were formed and that *A* had to be filled at least three times before the dye was completely expelled.

The following table contains the results of two series of determinations made with this apparatus in a shallow pond:

I. August 21. High Wind.		II. September 13. Light Breeze.	
Depth.	Oxygen Dissolved. Per cent. of Saturation.	Depth.	Oxygen Dissolved. Per cent. of Saturation.
5 feet	100.0	0 feet.	76.5
10 "	61.9	3 "	63.1
14 " (bottom)	32.4	6 "	53.4
		9 "	52.0
		12 "	44.2
		14 " (bottom)	44.1

Laboratory of the Springfield Water Commissioners.
Ludlow, Massachusetts, December 22, 1901.

BOOK REVIEW.

Cyanide Practice. By Alfred James. London, Spon. New York, Engineering and Mining Journal. 1902, p. 174. ill. Price, $5.00.

This book appears to be a semi-official statement of the history and development of the cyanide process under the Scotch parent company which is the owner of the Mac Arthur-Forrest patents. The author, who early became connected with this company and was in charge of the South African field, where the process first became a commercial success, records in this book mainly the results of his own practical experience. The book is therefore not a text-book, which necessarily must be to a large extent a compilation by competent authority, but a record of some of the investigations carried on by the company, which have made the process what it is today, and of the practical results attained in actual work. The results represent mainly the work in South Africa, Australia, and New Zealand. American practice is referred to only here and there. The whole manner of treatment indicates that the author presupposes the reader to be familiar with cyaniding in general, and to be prepared to appreciate the detailed practical information given; sometimes, however, he goes to the other extreme and gives elementary A B C which seems superfluous. The arrangement of the matter in the book cannot be called entirely satisfactory.

The first chapter, "Investigation of Samples," is very concise and fairly complete. It is taken from the author's paper read before the Institute of Mining and Metallurgy (1894–95. 3, 369), as is much of that which follows under the next two headings, "Plant" and "Extraction." The chapter on the plant gives many useful details; extraction covers crushing, leaching, chemical reactions, clean-up, fusion, administration, labor, capacity of labor and "Pointers," intended for all men connected with the works, from director to the foreman and the melter.

It sounds rather odd to have the author say (p. 56), in his discus-

sion of the precipitation of gold by means of zinc, that, with the possible exception of electricity, no other precipitant than zinc shavings had made any headway, when we consider the steadily increasing use of zinc fume (blue powder) in American cyanide plants.

The chapter on the "Treatment of Slimes" is very instructive. It shows why decantation has become the standard method of treatment where there is sufficient water to permit of its being applied. It is not quite clear why the author has given so much space to the Pelatan-Clerici process; he gives only American practice, and here the process can hardly be said to have achieved sufficient success to warrant such extended discussion. The Sulman bromocyanide process is discussed in full. In this country a good many experiments have been carried on with it, but it has failed to be adopted.

The chapters on "Dry Crushing" and on "Costs" are unsatisfactory, at least from the American point of view. The "Notes on Extraction Work" is in part a reprint ot the author's paper read before the American Institute of Mining Engineers (1897. 17, 278). On page 124 is another reference to the use of zinc dust, which is incorrect so far at least as one large American plant is concerned, the bullion of which is not so impure as when zinc shavings were used, and which requires less zinc dust for precipitation than it did shavings.

Under "Losses in Cleaning-up" there are found many interesting figures. It seems surprising to find the chemistry of the process discussed here (on page 147, the whole book having 172 pages). The chapter contains some new suggestions on determinations. Under "Recent Cyanide Practice" are given some more useful details. It is interesting to note, on page 160, that in South Africa there are found similar difficulties in precipitating gold from dilute cyanide solutions as in the United States.

To sum up,— The book will not delight the theoretical man nor the general reader; it will do excellent service to the engineer in charge of works, as he will find, what he usually looks for in vain, the record of experience of a man who has had to do much with the details of the work. Every cyanide company ought to have one or more office copies at the works, in order to afford employees every possible facility for becoming familiar with the contents of the book.

H. O. Hofman.

March 25, 1902.